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Biological Principles

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Biological Principles

A Critical Study

By

J. H. WOODGER

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1690

That which I have here to do is, to inquire whether, if it be the readiest way to knowledge to begin with general maxims and build upon them, it be yet a safe way to take the principles which are laid down in any other science as unquestionable truths; and so receive them without examination, and adhere to them without suffering to be doubted of, because mathematicians have been so happy or so fair to use none but self-evident and undeniable. If this be so, I know not what may not pass for truth in morality, what may not be introduced and proved in natural philosophy.'

Locke, Essay concerning Human Understanding, Bk. IV., xii. 4

1843

In the progress of science from its earliest to its more difficult problems, each great step in advance has usually had either as its precursor, or as its accompaniment and necessary condition, a corresponding improvement in the notions and principles of logic received among the most advanced thinkers. And if several of the most difficult sciences are still in so defective a state; if not only so little is proved, but disputation has not terminated even about the little which seemed to be so: the reason perhaps is, that men's logical notions have not yet acquired the degree of extension or of accuracy, requisite for the estimation of the evidence proper to those particular departments of knowledge.'

J. S. Mill, Logic, Intro. 6

1926

The progress of biology and psychology has probably been checked by the uncritical assumption of half-truths. If science is not to degenerate into a medley of ad hoc hypotheses, it must become philosophical and must enter into a thorough criticism of its own foundations.'

A. N. Whitehead, Science and the Modern World, p. 25

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PREFACE

Modern natural science may be likened unto a crab which has grown too fat for its shell. The process of ecdysis is slow and painful. The old shell, which has matured and hardened for some three hundred years, has done good service. No wonder the crab is loath to part with it. But it has already begun to crack, and some bits have even dropped off. What is to be done? Should the crab go on getting fat and take no thought for the raiment of the morrow? Or should it resolutely face the situation, heave off the remains of the old shell with a sigh, and set about making a new one in earnest? There is a great deal of uncertainty about the precise form and texture of the future new shell. But the evil day cannot be delayed much longer, and if it is put off too long the process of growth may suffer, or the whole may fall to pieces for lack of support.

The data of natural science constitute the meat. The shell in the parable represents the general philosophical background and the theoretical basis upon which the data have been systematized. Periods occur in the life of every branch of natural science when revisions and stock-takings of its foundations become necessary, but in recent years changes of a much more deep-seated and consequently far-reaching nature have been in progress, involving foundational beliefs which have scarcely been questioned before from the standpoint of natural science. So far this revolution appears to have had little influence on biology but has been confined to physics. Changes in the foundations of physics, however, may be expected sooner or later to concern biology, and especially those now in progress, which involve the foundations of our knowledge of nature in general. As long ago as 1887 Huxley wrote:

> 'Boyle did great service to science by his "Sceptical Chemist," and I am inclined to think that, at the present day, a "Sceptical Biologist" might exert an equally beneficent influence.'

So far as I am aware such a 'Sceptical Biologist' has never been written, at least in the English language, but the present

time appears to be a particularly favourable one for carrying out Huxley's suggestion, and the present work is a tentative attempt to do so.

In this Preface it will be desirable to explain the scope of the book a little so that a prospective reader may know what it will involve and how far it is likely to interest him. And it is important to emphasize the fact that such an undertaking as this involves a frame of mind and a way of thinking which is somewhat foreign, and perhaps even repugnant, to that proper to the scientific investigator. To write a book on how to conduct scientific investigations would be as silly as writing a treatise on how to paint pictures or write poetry. The 'Sceptical Biologist' has nothing to do with teaching investigators their business. He is concerned with interpretations: not with weighing empirical evidence upon which they are based, but with the most general assumptions, presuppositions, postulates, etc., which underlie them.

If we take such a scientific proposition as 'Adrenaline causes rise of blood-pressure' there are two directions in which our understanding can be extended. On the one hand we can repeat the experiment and try to determine more precisely what happens when adrenaline causes rise in blood-pressure. On the other hand we can ask ourselves more resolutely than the investigator usually needs to do what precisely we *mean*, what exactly we assert, when we say that adrenalin causes a rise of blood-pressure. Adrenaline is spoken of as a 'chemical substance' and rise of blood-pressure is referred to as a 'process'. What *exactly* is the difference between a chemical substance and a process, and what *exactly* is meant by saying that a substance causes a process? What in general is meant by substances and causes, and how is our knowledge of nature expressed and promoted by the use of these fundamental notions?

Now it happens (perhaps unfortunately if it suggests that they are unrelated) that when we follow the first course we are said to be pursuing 'science', and when we take the second we are pursuing 'philosophy'. It happens also that the people who follow the one alternative are not the people who follow the other. It results that the people who use these and other fundamental notions are frequently not at all clear about what precisely they mean by them. This may be expected sooner or later to have unfortunate consequences,

If such notions are used uncritically a time may come when, with the progress of investigation, and the increase of data obtained from sources far removed from ordinary experience, we may say more with them than we intend. We may suppose ourselves to be using them when we are in fact being misled by them. And yet, to be meticulously exacting about care in observation, and slovenly and careless in thought and the expression of thought is—to say the least—to spoil the ship for a hap'orth of tar. But this does seem to be the case in biology. Nothing is more striking in this science than the contrast between the brilliant skill, ingenuity and care bestowed upon observation and experiment, and the almost complete neglect of caution in regard to the definition and use of the concepts in terms of which its results are expressed.

Now if we are to call the investigation of such fundamental notions as causation, etc., 'philosophy', it is important to understand that it is only a part of philosophy. Some philosophers also employ the data of the various branches of learning in order to develop constructive schemes of thought about the general nature of existence and our own position in it. This is by far the most popular meaning of the word 'philosophy' and also the most popular *kind* of philosophy. Consequently it is the kind of philosophy which is most often meant when the term is used. Men of science usually take one of two attitudes towards this kind of philosophy. They either regard it with suspicion as a bad way of doing the same job as that upon which they are themselves engaged, or they regard it as a way of settling or at least studying certain interesting and important problems which are beyond the scope of natural science. Those who take the latter course often make a sharp distinction between philosophy and science—sometimes so sharp as to remove all possibility of any mutual influence between the two. Nothing that natural science discovers has any influence on their philosophy, because science is said to be 'abstract' or that it only deals with 'appearance'. Similarly, their philosophy never ventures to criticise or otherwise impinge upon their science. This may be called the 'Bradleyan view' of the relation between science and philosophy.[1] Those who follow the other attitude towards philosophy in regarding it as attempting to do what

[1] This view is expounded in F. H. Bradley's *Appearance and Reality*. London, 1908. pp. 443-5.

they are doing but by a wrong method usually have a constructive metaphysic of their own based upon natural science, often rather uncritically interpreted.

Between these two views (which only apply to constructive or speculative philosophy) the other kind of philosophy which is more modest in its aim (since it only tries to be clear about the exact significance of the fundamental notions and methods of inference which we employ in systematizing our knowledge) is apt to escape attention. And if it does receive notice it is usually mistaken for an idle discussion about words. But this is to confuse the symbol with what is thereby symbolized — a not uncommon mistake.

It seems, then, that the 'Sceptical Biologist' will not be able to avoid 'philosophy' in its more modest critical aspects, and at the same time he will be exposed to misunderstandings based upon the traditional scientific distrust and dislike of speculative philosophy. But, according to Professor Whitehead: 'To neglect philosophy when engaged in the re-formation of ideas, is to assume the absolute correctness of the chance philosophical prejudices imbibed from a nurse or schoolmaster or current modes of expression. It is to enact the part of those who thank Providence that they have been saved from the perplexities of religious inquiry by the happiness of birth in the true faith.'[1]

Broadly speaking we can now distinguish three main periods in the history of European thought: (1) that dominated by Aristotle, which came to an end with the Renaissance; (2) that dominated by Descartes, which reached its high-water mark in the nineteenth century; and (3) the transition period in which we now stand. The first and second periods may be described as dogmatic in the sense that they had cut and dried answers to most questions; the third may be called critical inasmuch as it has not so far been dominated by any one constructive scheme, but has chiefly been concerned with a general overhaul and stock-taking of fundamental ideas. Those who dislike dogmatism and delight in 'transition periods' will be glad to be alive in this new springtime of thought—when 'all the wood stands in a mist of green and nothing perfect'. But, as I have said, the only science which has been influenced to any extent by this change so far is physics. It is in the mathematical physicist that we most

[1] *The Principle of Relativity*, Cambridge, 1922, p. 6.

often find a union of the man of science with the critical philosopher, although by no means all physicists are critical. Biology is still in that phase which, according to Comte, all sciences pass through, and which he called the metaphysical stage. Among English biologists the late William Bateson seems to have come nearest to understanding the critical standpoint. Consequently the need for a critical review of its principal difficulties and fundamental notions is perhaps greater in biology than in any other science.

The mutual relations of the two great branches of inquiry—those of critical philosophy (i.e. logic and theory of knowledge or epistemology) on the one hand, and the experimental investigation of nature, on the other, as seen in the history of physics, have been well sketched by Professor E. Cassirer in the following passage:

> 'The working together of the two points of view has always come to light with special distinctness at the decisive turning points in the evolution of theoretical physics. A glance at the history of physics shows that precisely its most weighty and fundamental achievements stand in closest connexion with considerations of a general epistemological nature. Galileo's *Dialogues on the Two Systems of the World* are filled with such considerations and his Aristotelian opponents could urge against Galileo that he had devoted more years to the study of philosophy than months to the study of physics. Kepler lays the foundation for his work on the motion of Mars and for his chief work on the harmony of the world in his *Apology for Tycho*, in which he gives a complete methodological account of hypotheses and their various fundamental forms; an account by which he really created the modern concept of physical theory and gave it a definite concrete content. Newton also, in the midst of his considerations on the structure of the world, comes back to the most general norms of physical knowledge, to the *regulæ philosophandi*. In more recent times Helmholtz introduces his work, *Über die Erhaltung der Kraft* (1847), with a consideration of the causal principle as the universal presupposition of all "comprehensibility" of nature, and Heinrich Hertz expressly asserts in the preface of his *Prinzipien der Mechanik* (1894), that what is new in the work and what he values is "the order and arrangement of the whole, thus the logical, or, if you will, the philosophical side of the subject." But all these great historical examples of the real inner connexion between epistemological problems and physical problems are almost outdone by the way in which this connexion has been verified in the foundation of the theory of relativity. Einstein himself . . . appeals primarily to an epistemological motive, to which he grants, along with the purely empirical and physical grounds, a decisive significance.'[1]

[1] *Einstein's Theory of Relativity considered from the Epistemological Standpoint*, Eng. trans. by W. C. and M. C. Swabey. Chicago, 1923, pp. 133-4.

Those biologists who admire the methods of the physical sciences would do well to consider carefully the implications of the above passage for biology.

What I have attempted to do in this book is to give a general sketch of the whole wide field which is involved when we try to dig down to scientific foundations. Consequently the proportion of 'biology' to 'philosophy' is very small. The General Introduction is devoted to introducing the topics to be discussed in an elementary way, and to removing certain difficulties which would be liable to arise later and confuse the issues. Part I deals with the general problems of the theory of knowledge which are involved in the interpretation of the results of any branch of natural science, although I have used biological examples to illustrate general principles as far as possible. In Part II I have picked out what appear to be the most general and most deep-seated difficulties and peculiarities of biological knowledge, and I have attempted to use the results of Part I to elucidate them. But it is necessary to repeat that even in this part the critical biologist is not primarily concerned to discuss rival *biological* theories, or to attempt to decide between them, on the basis of an estimate of the relevant empirical data. His duty is simply to examine the logical procedure and ontological assumptions involved in them, and to show how far the difficulties ordinarily felt in regard to such rival theories depend upon such assumptions. Those philosophers who interest themselves in the methodology of natural science usually confine their attention to physics. I hope they may find something of interest in Part II of this book which will persuade them to devote some attention to the interesting and largely unexplored requirements of biological thought.

In the course of Part II I have ventured to put forward some suggestions towards a resolution of certain of the traditional biological conflicts which are there discussed, but my primary aim has been to introduce and expound a new[1] method of approach to such questions—a method which has not yet received the care and attention among biologists which it deserves. Consequently it will be a matter of little importance what becomes of any constructive suggestions which may here be brought forward (and this applies especially to Chapter IX) so long as the point of view from which the difficulties themselves have been approached has been understood. But this

[1] 'New,' that is to say so far as English biology is concerned.

can only be done if the reader is prepared to stand aside, so to speak, from his ordinary scientific way of thinking and subject it to a critical and disinterested examination.

Criticism in the common meaning of the term may be either hostile or friendly. The hostile critic has an axe to grind. Criticism as here intended (in the sense in which it was first introduced into philosophy by Kant) is a disinterested examination of traditional conflicts with a view to the discovery of their roots, and the removal of difficulties created by an uncritical use of the notions of unreflective thought. Criticism of this kind is friendly in so far as it seeks to remove difficulties and not merely to point them out. It has no axe to grind because it does not aim at speculative constructive schemes. It deals only with foundations, leaving the superstructure to others. In the words of Locke: 'It is ambition enough to be employed as an under labourer in clearing ground a little, and removing some of the rubbish that lies in the way to knowledge.' A great deal of the discussion that goes on in biology around the traditional controversies seems to be vitiated partly because it is not taken deeply enough, partly on account of the almost universal neglect of the elementary precaution of defining the meaning of the terms used, and partly because it seems always to be conducted from the standpoint of one side or the other rather than from one of neutrality. These are three defects which I have tried to remedy in what follows. One consequence of these defects is that appeal is made to *convictions* instead of to *reason*. In regard to the celebrated quarrel between 'mechanists' and 'vitalists', for example, it is not a bit of use appealing to 'instincts' and 'intuitions' to reveal the shortcomings of one side or the other. It is necessary to analyse the precise nature of the strong and weak points of each side so that we can make a rational judgment of their relative merits. What is here offered, therefore, is a study of the central problems and basal difficulties of the biological sciences not from the standpoint of one or another of the traditional theories, but from that of disinterested critical reflexion, utilizing any aid that modern investigations of this kind in other spheres may be able to offer. And if, in relation to a particular controversy, I have criticized one side more than another I would ask the reader to believe that this is not because I wish to favour one side more than another, but because one seems to be in more

need of critical examination than another, and from considerations of space it is necessary to concentrate on points which require most attention.

In the search for aids to the elucidation of the difficulties peculiar to biological thought I have turned modern developments in many branches of inquiry to account—particularly in logic, theory of knowledge, and physics. Accordingly it has not been possible in Part II to pursue the problems dealt with there in any great detail. But I venture to hope that the method of approach here advocated will be capable of much further development, and that the reader who is interested in this aspect of biological inquiry will be aided and encouraged to undertake such further development. In this connexion I would especially draw attention to the need for a critical analysis of biological concepts with a view to freeing them from historical accretions which are not in harmony with modern knowledge, and to the need for more boldness and originality in developing new ones. I have devoted special attention to the important notion of 'organization' which seems to have been singularly neglected. Another topic of equal importance is the ancient antithesis between 'structure' and 'function' to the removal of which, as I have tried to show, modern developments appear to furnish the key. The consequences of a more detailed working out of these developments in relation to this and other biological problems should be important and interesting. But I would point out that it will be quite impossible properly to follow the arguments of Part II if due attention is not paid to the difficult but quite fundamental problems discussed in Part I. I must also state clearly at the outset that what is here offered does not profess to be in any sense exhaustive or complete. Even in regard to some of the most general problems which I have singled out for discussion it has not been possible to do more than analyse and state the difficulties clearly and so to leave them for the present. This applies with special emphasis to Chapters IX and X. My feelings on this point are echoed in the words of the preface of another author: 'What I have here in some measure set in order is adjoined on all sides by thickets abounding in monstrous doubts and difficulties. There are complications which I have not followed out, assumptions which I have not followed back, and after-thoughts which I can already anticipate.' But something is accomplished even

when difficulties are merely brought to light and stated clearly, and if more people are persuaded to set about removing some of the 'monstrous doubts and difficulties' with which biological thought abounds, the purposes of this book will have been accomplished. It is necessary first to make clear to biologists the existence of these difficulties, and then to bring home to them the nature of the intellectual weapons they are using and, by showing them something of their origin and of the uses for which they were originally intended, to persuade them to consider more critically their use and limitations in the biological field. Having in this way fostered a wholesome spirit of scepticism and a dissatisfaction with their 'current modes of abstraction' on the part of those interested in theoretical biology, the desire for something more satisfactory will lead to the exploration of other possibilities, and the exploitation of the rich potentialities of human thought which have not yet been drawn upon.

My indebtedness to other authors will be sufficiently plain from the references given in the text. I have learnt most from the writings of the Cambridge school of logicians, particularly from the works of Messrs. C. D. Broad, W. E. Johnson, G. E. Moore, Bertrand Russell, and A. N. Whitehead. And of these the first and the last have been of most help from the biological point of view. Professor Whitehead's views are specially sympathetic to a biological application. I am greatly indebted to Miss L. S. Stebbing for twice reading the book in manuscript from the logical standpoint. I have profited much from her criticisms and advice. To Dr. E. S. Russell I am indebted for reading the manuscript from the biological point of view and making a number of helpful criticisms. Similarly, Mr. G. C. Robson kindly read Chapters VI and IX. Acknowledgements are due to the editor of *Science Progress*, for permission to make use, in Chapter IV, of parts of a review which has already been published in that journal. Finally, I am obliged to my wife and to Dr. S. Wright for help in proof-reading.

J. H. WOODGER.

Middlesex Hospital, Medical School,
March, 1929.

'If anyone does not care for knowledge for its own sake, then I have nothing to say to him; only it should not be thought that a lack of interest in what I have to say is any ground for holding it untrue.'—G. E. Moore.

BIOLOGICAL PRINCIPLES

GENERAL INTRODUCTION

BIOLOGY is a science of antitheses. If we make a general survey of biological science we find that it suffers from cleavages of a kind and to a degree which is unknown in such a well unified science as, for example, chemistry. Long ago it has undergone that inevitable process of sub-division into special branches which we find in other sciences, but in biology this has been accompanied by a characteristic divergence of method and outlook between the exponents of the several branches which has tended to exaggerate their differences and has even led to certain traditional feuds between them. This process of fragmentation continues, and with it increases the time and labour requisite for obtaining a proper acquaintance with any particular branch. But whereas in some sciences this process has been accompanied by the attainment of generalizations which have tended to knit the several branches into a single whole, in biology the disruptive process has not been compensated by the help of any principle of such unifying power, and the possibility of a unified biology seems to recede more and more from our grasp.

At the bottom of these difficulties we find the fundamental antitheses of biological thought. Structure and Function, Organism and Environment, Preformation and Epigenesis—these are some of the antitheses which determine the lines along which biology is divided in more senses than one. There are, moreover, lines of cleavage which cut deeper still. Where, in the physical sciences, shall we find such a total divergence on a fundamental matter of principle as that which separates those who profess some form of 'vitalism' from the exponents of 'mechanism'? This ancient controversy continues as

nauseam and shows no sign of abating. Biology has been cleft by this antithesis at all stages of its history. The same arguments are repeated by successive generations without any resolution of the conflict. Each side belabours the other without—if we may judge by the confidence with which each continues to assert its convictions—making any impression on its opponents.

Involved in this controversy are others which have their roots in the antithesis between 'Body' and 'Mind'—an antithesis which is not a purely domestic one for biology but which links it with psychology. Much help has been furnished by physical and chemical knowledge in the investigation of biological problems. This aspect of biological inquiry stands in no danger of neglect. It enjoys a wide popularity to-day, especially in view of its applications in medicine. But although biology appears to have clear points of contact with psychology this relation receives comparatively little attention. Far from psychology being *sought* as an aid and ally, it seems to be *avoided* as more likely to create difficulties than to remove them. And yet, in the human organism at least, the spheres of physical, biological, and psychological science seem in some way to coalesce, and the critical inquirer should not allow this unique circumstance to be set lightly aside. The origin and significance of the prevailing point of view call for his attention.

But without going beyond the boundaries of our own science there are problems enough when we try to bring the results of its main branches together. The general theoretical results which have been reached by investigation along the lines of physiology, experimental morphology, genetics, cytology, and the older descriptive morphology are extremely difficult to harmonize with one another, even although, for various reasons, these difficulties are not apparent on a *prima facie* view. As soon as we do attempt such a synthesis we are confronted with contradictions which appear to rest on the fundamental biological antitheses. Instead of a unitary science we find something more approaching a 'medley of *ad hoc* hypotheses'. Moreover, the fundamental cleavages of opinion are reflected in the exposition of the results of this department of science according to the beliefs to which a given author subscribes. An author will tend to give more emphasis to those facts which support the view he favours than to those

which are antagonistic. Consequently the reader of biological literature is compelled to bear this constantly in mind and must seek to correct one exposition with another. And this presupposes some acquaintance with the antitheses of biological thought and their consequences. How is it possible to improve upon such an unsatisfactory state of affairs? Would it not be possible to take up an attitude of neutrality towards the traditional conflicts in order to study the nature of the antitheses upon which they appear to rest? If we do this we may find that the roots of these antitheses themselves do not lie wholly within biology but are to be sought elsewhere, and in that case where are we to seek them? In other words: what is to be our standpoint for such an inquiry?

If we ask ourselves what exactly we are doing as biologists we may find some indications of how to set about answering these questions. It will be generally agreed that biologists are primarily concerned with the investigation of those constituents of the world which we call animals and plants. Biology, it will be said, is that branch of science which deals with these things. We are therefore referred to something wider, called science, of which biology is a part. What, then, is science? The word has a diversity of meanings. In one sense—as when we speak of this or that science—it stands for a body of knowledge: a systematized body of propositions about some subject matter or other, enshrined in books or in the minds of individual men. We ought, therefore, to speak, not of science but of the sciences, or, if the term is used in the singular, it would seem to be a collective term for all the branches of science taken together. But the word science is by no means universally employed simply as a general name for all systematized knowledge. It is doubtful, indeed, whether it is ever used in this sense at the present day. Often it appears to be used not as a name for *knowledge* about some subject matter but as a name for a particular *attitude* or activity towards nature. Not infrequently it appears to be confused with a certain *theory* about the nature of existence in general—the theory more properly referred to as 'naturalism'. Again, many writers seem to restrict the term science so far as to include in it only such knowledge as is systematized

with the aid of mathematics, in which case a great part of biological knowledge would be excluded. But by far the commonest usage is in its application to what is also called 'natural science', the adjective 'natural' frequently being omitted. If we follow this usage we shall be restricting the application of the term to knowledge about nature, instead of making it equivalent to the whole of knowledge—unless, of course, we believe that 'nature' is equivalent to all that there is to know, and in that case science, natural science, and knowledge would be synonymous terms. But many people would not agree to this synonymy. They would say that knowledge is wider than natural knowledge, since there are branches of science which do not have 'nature' for their object in the same sense as physics, for example, does. To clear up this difficulty we should obviously have to decide what we mean by 'nature'. But for the present we are trying to be clear about what we mean by science. Biology, we say, is a branch of science, and science, whatever else it is, is knowledge; whether it is to be regarded as a part or the whole does not concern us for the present. Now the possibility suggests itself that the roots of those biological antitheses do not lie wholly in the nature of the *organisms* and are not, therefore, to be studied as part of the subject-matter of biology, but that they arise partly out of the nature of *biology* itself as knowledge. This further suggests that by the study of biological thought from this point of view we might throw some light upon the nature of biological controversies. What, then, is knowledge? Biologists often speak of our knowledge of this or that, but they rarely discuss knowledge itself. They are concerned with *getting* knowledge *about* animals and plants. They are interested in such knowledge, but not in it as knowledge. They may, in fact, forget that they are dealing with knowledge at all, and suppose themselves to be dealing not with knowledge but simply with animals and plants. But this is clearly not the case, otherwise what would distinguish a biologist from a landscape painter? The latter might also claim to be dealing with animals and plants, and so he is—but not by way of knowledge. Both are dealing with animals and plants but they are exercising different activities, the biologist's activity is primarily intellectual and its outcome is knowledge. This being understood we now have to inquire what is involved in biology as a branch of knowledge, in the

hope that such inquiries may help us towards a better unification of biology.

I have referred to a science as a 'systematized body of propositions about some subject matter or other, enshrined in books or in the minds of individual men'. This will serve as a provisional analysis of what is involved in knowledge, and as a starting point for further inquiry. We can now (i) examine some particular instances of biological *propositions*, then try to determine (ii) how they are systematized, and (iii) consider how they are related to minds. What the propositions are about we need not now inquire because we know that this forms the subject-matter of biology, but we shall have to study (iv) the relation of propositions to this subject-matter as well as their relation to minds. It seems from this that propositions have a double relation: on the one hand to minds, and on the other to subject-matters. A detailed study of these questions will form the topic of Part I. For the present it is only necessary to pursue them a little further.

(i) The following propositions are taken at random from a biological book:

> 'Isolated rudiments of the eye, the nose, and the ear of the chick differentiate independently on the chorio-allantoic membrane. The same is true of rudiments of pronephros, mesonephros, neural crest, liver, pancreas, intestine.'

These will illustrate some of the properties of propositions in general. Something is *asserted* of something else. We are told that certain objects (isolated eye rudiments) *do something* (differentiate independently) in a certain place (chorio-allantoic membrane). The second of the two sentences quoted reminds us that such assertions make a certain claim—they claim to be *true*. Thus biology consists in making true assertions about organisms, or parts of organisms. It will be noticed that although explicit reference appears to be made to a certain *place* no such reference is made to *time*. The proposition asserts that whenever certain objects occur in a certain situation a certain event happens, and it is therefore implied that *when* it happens is a matter of indifference. In a sense this is also true of place. The proposition says that the eye rudiment is in a certain place—the chorio-allantoic

membrane, but it does not say where the chorio-allantoic membrane has to be. Is this also a matter of indifference? The author assumes quite legitimately that the reader knows that what is meant is that the membrane is part of a chick, that the chick is alive and in an egg-shell, and that the egg-shell is either in an incubator or under a hen. Is it then a matter of indifference where the hen or the incubator is? Could they, for example, be at the North Pole? What is usually meant is that it does not matter *where* the membrane is so long as certain unspecified, because unknown, *conditions* are realized. What the proposition wants to assert is that *if* such conditions are realized the assertion will hold good, no matter where or when they happen to be realized. Thus the proposition, in spite of the form in which it is stated, is really *hypothetical*, and involves reference to an ever widening circle of conditions—recalling the 'conditions' which prevented the old woman from driving her pig home from market—about which it says nothing. We cannot say that where and when do not matter because at every time and place of which we have any knowledge *some* conditions prevail, and there may be times and places at which hens and incubators cannot function. It seems to be begging the question to say that, *if* the same conditions are realized when *you* try the experiment as were realized when *I* tried it, then you will get the same result, because I do not know what conditions were realized in my case and you will not know what conditions will have been realized in your case. But if two experiments give the same result it is said that it is because the conditions were the same and if the results are different this is ascribed to the supposed fact that the conditions were different.

The same difficulty arises in connexion with the *objects*. We know, of course, that only a certain limited number of eye-rudiments have actually been placed in the situation mentioned, in a certain laboratory at a certain time. But the proposition does not restrict itself to those objects, any more than to that laboratory or to that time, and it may be asked: How is it possible to make true assertions about other eye-rudiments when you have only observed a few? What does it mean to claim *truth* for such propositions? If we knew that all eye-rudiments and all chorio-allantoic membranes differed from one another only numerically then it would suffice to try the experiment once only, but we are told that

'variation' is an important and common characteristic of living things—so important and frequent that Darwin founded a theory of evolution on this fact as a basis. Thus we not only do not know that all objects of the kind mentioned are merely numerically different, but we should be contradicting an important general biological principle even in assuming that they were.

What, then, do we mean in claiming truth for such a proposition? If the author had merely said that certain events had happened in certain objects at certain times and places we should know that by this being true simply meant that such events had actually been observed, and a reputable investigator who made the statement would have no difficulty in convincing reasonable people of its truth by showing the specimens he had obtained. But such a proposition would be an historical proposition—it would merely record a fact. The biological proposition clearly wants to do more than that. For reasons already noted we could not test this assertion by doing the experiment ourselves because this would still leave an enormous number of cases still untested, and if the experiment were unsuccessful it could still be said that this was because the conditions were not favourable, as so frequently happens in biological experiments. But if a number of careful operators repeat the experiment unsuccessfully then we are inclined to say that the proposition is false, and we should reject it as worthless (although it would still remain true as a historical record). Thus the number of times the experiment has been repeated is considered to be of importance. But even if these further experiments confirmed the original proposition we should still be in doubt about the time question. How far, for example, into the past may we suppose such a proposition to have been true? When we have to invoke present conditions about which we know so little, what can we be justified in saying about past conditions about which we know nothing? This is evidently a difficulty which we ought to bear in mind when we make statements (sometimes using the word 'must') about organisms at enormously remote epochs.

Yet in spite of all these difficulties the procedure of natural science seems to work well enough to sustain our hopes that our inductive propositions say something meaningful and form a worthy foundation for the erection of speculative theories.

These difficulties, which come to light as soon as we begin to reflect, and which appear to be particularly acute in the biological sciences, will have to be removed if we are to find a logical justification for our scientific procedure, if, that is to say, it is to be valid.

(ii) What do we mean by systematized propositions? By a body of propositions being systematized we mean that it possesses some sort of order, unity, form, or organization. What, then, determines this form? Upon what principles does it rest? Here we have to remember the double relation of propositions to minds on the one hand, and to a subject-matter on the other. We may expect that both of these relations will have an influence upon the structure of our knowledge. It seems clear enough that the character of the subject-matter will determine, in part at least, the mode of sub-division, for example, of a science. But it is also equally clear that minds have some way in the matter. Some thinkers have even gone so far as to suppose that *all* the systematization our knowledge possesses is the 'work of the mind'. But quite apart from such an extreme it is evident that minds make certain *demands* which knowledge is expected to fulfil, such as generality, simplicity, etc. These are characteristics of propositions. But before all else we demand that our knowledge shall be free from contradiction and that it shall be true. The nature of the subject-matter, however, may be such as to set certain limits to the attainment of some of these ideals. What we discover about organisms, for example, conflicts very much with the demand for simplicity. 'Seek simplicity', says Professor Whitehead, 'and distrust it'. Consequently, if we find that the nature of the subject-matter conflicts with a demand we shall feel that in order to reach the ideal of truth we shall have to relinquish the demand in question. It will be generally agreed that in science we are not concerned with making our knowledge conform to our wishes but primarily in assuring ourselves that it tells us *what is in fact the case* in the particular sphere in which we are interested.

Then there are certain extremely general notions such as that of a 'thing' having qualities and properties, and the notion of cause, which play a fundamental part in the organization of knowledge. Biology too, as a branch of natural science, is under the dominion of those most pervasive features of our experience which we call space and time. The study

of these and of their significance for science has occupied a great deal of attention in recent years. Closely connected with them is the problem of induction to which we have already referred. Moreover the whole of our scientific procedure involves certain universal assumptions and beliefs which require examination. A study of these matters—all of which are implicated in the 'principles of systematization'—will be undertaken, from the standpoint of biology, in Part I. We shall see that an examination of these principles of systematization is of great importance in understanding the biological antitheses. The traditional contrast between structure and function, for example, which may seem at first sight to be determined solely by the nature of the subject-matter will be shown to depend on our attitude to some of the general concepts to which we have been referring.

(iii) With regard to the relation of minds to propositions it will suffice for the present to say that, except for an extreme behaviourist, propositions are the outcome of mental operations of the kind called intellectual. If you are asked what is the sum of 25 and 18 you will probably say, after an interval, 43. And what you did during part at least of that interval would be called, by most people, an intellectual operation. The propositions of a science refer, we said, to a subject-matter, and hence minds are indirectly related to the subject-matter through propositions. But minds are also directly related to subject-matters. In the case of natural science this relation is always in the first instance through sense-perception, and the relation is usually described as one of awareness. Perception is also regarded by most people as a mental operation. Thus in natural science we first become aware of our subject-matter through perception, and as a result of subsequent operations we give utterance to propositions about it. The nature of perception and its relation to knowledge will be the first topic for Part I. But intellectual operations are not the only mental processes. When anyone 'gets heated' in an argument it is usually believed that he is the subject of mental processes of the kind called affective or emotional. Moreover, some people, in the course of an argument, may deliberately *appeal* to such processes in their hearers in order to influence the kind of propositions they will make about certain subject-matters. Thus there are other mental processes which may interfere with the intellectual

operations, and in discussing a theory we ought to take into consideration the possibility of such influences in its composition. Such possibilities will be discussed in Part I. under 'subjective factors' in knowledge.

This first brief examination of knowledge brings us back to the question whether science as knowledge could be regarded as equivalent to 'natural' science. We have been taking *knowledge itself* as our subject-matter—making *it* the object of inquiry. Now knowledge is not usually regarded as a 'natural object' in the same sense in which a rabbit is regarded as a natural object, and hence if we are going to use the term natural in its common sense, natural science will not be equivalent to all knowledge. But the branch of science which takes knowledge for its object is usually called logic, so it seems that what we are seeking as a basis for the study of the biological antitheses is the logic of biology. Are there other branches of knowledge which do not come under 'natural' knowledge? The reference to minds suggests that a systematized knowledge of such objects would also be a science, but would it be a 'natural science'? We see that this expression 'natural science' is constantly forcing us to ask what we mean by nature in order to determine whether a particular bit of subject-matter is to be referred to nature, and so to natural science, or not. But it is evident that to decide such a question we should have to make a survey of the subject-matters of a number of sciences and study the relations between them.

It might seem that such questions are of no importance for our present purpose. But it is absolutely essential at this stage to consider the relations of the various branches of knowledge to one another in order to avoid confusions and difficulties which would otherwise present themselves later. We have seen that each science has a certain field marked out for it by its subject-matter. What is the relationship between the subject-matter of physics and that of biology? or between the subject-matter of the science which studies minds and that which studies animals? Most people believe that although there are many sciences there is only one universe, and that the results of all the sciences taken together ought to tell us

something about that universe and about our own position in it. Moreover it is felt that, although there is such diversity between different sciences, and so much difficulty in finding any principle of unity between them—often we find contradiction and incompatibility instead—it is felt that, in spite of all this, there is unity of some sort and non-contradiction in the universe itself if only we could discover it. But if we are *in* one science how can we answer such questions? Is it even possible for one man to know the field of his own science properly, let alone enough about another to talk about the relationship between the two? Even in a single science there are many sub-divisions, and in order to *add* to the knowledge of a particular division it is necessary, at the present day, to know a great deal about it, and this usually means that there is no time left to know much about the others. What does the average zoologist know about botany? or the average animal morphologist know about animal physiology? A specialist has been described as one who knows more and more about less and less. But we have seen something of what 'systematized' means, and how each science strives after a theoretical interpretation of its own field in which the details will be organized in accordance with certain principles. Most people will agree with Poincaré that a collection of facts no more makes a science than a heap of bricks makes a house. Thus there is not only a process of adding to knowledge but also one of organizing it. But a specialist in one branch would only be able to do the organizing of the branch with which he was familiar, he would not have time left for attending to other branches if he were also engaged in adding to his own. One can imagine a highly perfected science in which it would be necessary to have not only 'adders' who also did the organizing each in his own sub-division, but also 'organizers' who did nothing but study the mutual relations between the sub-divisions. It might be necessary to have hierarchies of 'organizers' to deal with groups of sub-divisions, according to the degree and manner in which the subject-matter was divided. The only science which approaches such a state at present is physics, and here the 'organizers' have been mathematicians who perhaps have never been in a laboratory in their lives. One thinks of Leverrier predicting the discovery of a new planet and being too bored—so the story runs—even to look through the telescope when his prediction was verified.

Moreover in physics what I have been calling 'principles of systematization' have been the subject, all through its history, of lively discussions. When we turn to biology we find, as I have said, a large number of branches pursued by specialists who are too busy making new additions to knowledge each in his own branch to attend to other branches, still less to devote any time to the study of the principles of systematization. Moreover the difficulties of biology are still further exaggerated by those deep cleavages of opinion to which reference has been made and with which every biologist is familiar.

Now the question: What do we mean by nature? is one of those which appear to necessitate a consideration of more—many more—than one science. It could not be decided by a *specialist* in one science. All *he* could tell us would be about the present state of *his* science, or rather his particular branch of that science. It is also clear that what he had to tell us would be valuable just in proportion as he was *disinterested* in the general questions. His function is just to tell us what he finds to be the case in his own sphere with the minimum of distortion. Thus it would seem that there ought to be a most general science, not immersed in a particular subject-matter, but dealing with the relationship between the various special sciences, and trying to synthesize their most general results. And the function of such a general science would not necessarily be exhausted by attempting to make a synthesis of knowledge. It might also be able to help the special sciences by pointing out contradictions between them, and thus suggesting new lines for investigation with a view to their removal. It is obvious that such a general science would differ in a number of important respects from a special science and would probably require a different type of mind for its pursuit. It could not, for example, make experiments because it would be dealing with the whole. It would be utterly dependent for its data upon the special sciences, but it would also preserve a strict impartiality between them, just taking their data as it receives them and making what it could of them by way of synthesis. Its success would depend on the care taken in making the subsidiary syntheses in each special science, since it would have to assume that such syntheses had been carefully and disinterestedly carried out. Being dependent in this way, the conclusions of such a universal science could not be more probable than those of the sciences upon which it was based.

Like their conclusions its own would, therefore, always be tentative and subject to revision.

Such a general science would also serve as a corrective to certain defects which inevitably attach to the procedure of the special sciences. The whole success of the latter depends upon their way of dealing with their problems piecemeal—postponing difficulties until the easier tasks have been completed. In other words they proceed by abstraction—a most important feature which will have to be considered in detail later. Now in our universal science this would obviously be impossible. Since it has undertaken to give some account of the whole it could not abstract but would have to ensure that the consequences of one mode of abstraction were duly compensated by the use of others. It thus becomes, in the words of Dr. Broad, the residuary legatee of all the difficulties which the special sciences have found it convenient to neglect.

From the earliest days of systematic reflexion there has always been an ideal of such a science and it is commonly called metaphysics. Whether it has always supposed its task to be such as I have depicted it may well be doubted. All I have been doing has been to state my purely personal opinion about it. There appear to have been two principal types of metaphysics in the past: one, which we may call the a priori type, which works deductively from agreed 'first principles': the other, 'empirical', attempting to reach a general synoptic interpretation of the results of the special sciences. Among recent thinkers the work of M'Taggart might be cited as an example of the first, and that of Professor Alexander as an example of the second. But such a division makes the situation appear to be simpler than it really is because it is difficult to be purely a priori or purely empirical. Now from time to time we hear about a conflict between 'science' (meaning natural science) and metaphysics. There seems to be a tradition among men of science that there is something disreputable about metaphysics—something which makes it a topic which it is desirable to avoid. But people who hold this do not by any means make it quite clear what they mean by metaphysics. This is not a satisfactory state of affairs, because if you do not know clearly what metaphysics

is you may easily fall into it unawares, and if metaphysics is something it is desirable to avoid it will be a misfortune to fall into it unawares. It seems very frequently to be the case that what a person means by metaphysics is the opinion on certain topics of other people who do not agree with him. His own opinion he calls 'science'. This, clearly, will not do. It reminds us too much of the story of the man in *Punch* who was explaining the difference between 'doctrine' and 'dogma': 'What he thocht richt was doctrine, and what the ither ying thocht richt was juist dogma.' If metaphysics is defined in this way there will be as many definitions of metaphysics and of science as there are different opinions. It will, moreover, have the serious consequence of leading to perpetual confusion between a particular science and metaphysics in the sense in which I have described it. It will usually be found that people who make the division in the above way have already made up their minds on certain metaphysical questions without being aware of the fact—owing to a failure to distinguish clearly the problems and methods of science, on the one hand, and metaphysics on the other. From what has been said it will be clear that they differ in many important respects.

Put in the way I have attempted to put it, it seems absurd that there should be any conflict between metaphysics (in the sense of a most general synthesis of the special sciences) and a special science which furnishes the general one with data. It would seem, rather, that they should be mutually complementary. But there certainly is a tradition of such a conflict, just as there is a tradition of a conflict between 'science' and religion—as though science were always quarrelling with something or other. How has this tradition arisen? Such quarrels—like disputes between nations—not infrequently result from lack of mutual understanding, and in a world of knowledge divided into thought-tight compartments it would not be surprising to find such a lack.

Metaphysics—like politics and theology—is one of those topics upon which many people hold very definite opinions without considering it necessary either to have any special preparation, or to attach much importance to the views of those who have devoted themselves especially to them. No-one dogmatizes about organic chemistry: it is felt that unless you are yourself a chemist it is inadvisable to hold very con-

fident opinions on such a subject based solely on your own resources. But with metaphysics it appears to be otherwise. This is partly because most people are interested in the general problems of existence, and do in fact take up some sort of attitude towards them, whereas only a few people are interested in organic chemistry. But it is also because problems in the special sciences can frequently be put to the experimental test and there is a possibility of your erroneous opinion being refuted. Metaphysical opinions, from the nature of the case, cannot be tested in this way. Consequently whereas chemistry will be able to give a positive opinion upon many questions, metaphysics would only be able to offer a number of alternatives. But is this a sufficient reason for believing that the opinion of, say, an exponent of a special science is as good as that of one who has devoted himself to such questions? Medical authorities are notoriously divided in their opinions, and these too are often of such a nature as not to admit of being settled by an experimental test. And yet we often consult them in spite of this, feeling that it is better to trust someone who has devoted attention to such topics rather than to rely on our own resources. Thus the fact that so many people hold decided opinions about metaphysical problems without considering it necessary to have any special knowledge of the subject, may be due to ignorance of the nature of such problems, and also to the existence of non-intellectual factors influencing their beliefs. There is a confusion between the practical attitude of everyday life which cannot wait to have its problems 'solved' for it by the laborious processes of the intellect, and the purely intellectual attitude whose concern is solely for the truth and which, if it is to be faithful to this demand must needs have infinite patience.[1]

It is not difficult to see other reasons for the existence of the traditional conflict between natural science and metaphysics, some resulting from faults on one side, and some from faults on the other. First, there is diversity of *interest*. The type of mind which is attracted to a special science will differ from one which is attracted to metaphysics. We all like to feel that what we are doing is important, and we are apt to think that what we do not understand or are not interested in is not important. It will be easy for one who has

[1] Cf. G. F. Stout, 'In intellectual morality the fundamental virtue is patience.' *Analytical Psychology*, I. 242.

the gifts and good fortune to make brilliant discoveries about which a great many people can be thrilled, to feel more important than one who generalizes about other peoples' discoveries, and whose theories may only be understood by a few. But there are people who are more gifted for making general interpretations than particular discoveries, and it seems absurd to make distinctions about the relative importance of workers in different branches, or at different levels of generality, of the fabric of knowledge, when they are so mutually dependent upon one another. It is largely because this mutual relation is so little realized that such distinctions are made. But the history of science is abundant with instances of this mutual interdependence of theory and investigation, and Bacon has beautifully expressed his ideal of what their relations should be:

> 'Those who have treated of the sciences have been either empirics or dogmatical. The former like ants only heap up and use their store, the latter like spiders spin out their own webs. The bee a mean between both, extracts matter from the flowers of the garden and the field, but works and fashions it by her own efforts. The true labour of philosophy resembles hers, for it neither relies entirely nor principally on the powers of the mind, nor yet lays up in the memory the matter afforded by the experiments of natural history and mechanics in the raw state, but changes and works it in the understanding. We have good reason therefore, to derive hope from a closer and purer alliance of these faculties [the experimental and the rational] than has yet been attempted.'—*Novum Organum*, lib. I, 95.

When we consider how easy it is for a specialist to get his own little bit of experience out of perspective an intellectual world peopled only by specialists becomes a horrible object to contemplate, and we turn a more sympathetic eye in any direction which offers a counterpoise to the inevitable defects of professional specialism. The need for the alliance of which Bacon speaks is even more urgent at the present day than it was at the time in which he wrote, when it was still possible for a man to 'take all knowledge for his province.' To-day when such a thing is utterly impossible the need for mutual help and understanding becomes imperative if knowledge is not to degenerate, as Professor Whitehead says, into 'a medley of *ad hoc* hypotheses' and remain so. The belief that it was possible to attain to certainty upon metaphysical questions by merely thinking about them without regard to empirical data does not appear to have been at any time so

prevalent as one would suppose from those who write about a conflict between science and metaphysics. Each age does the best it can with the data at its disposal, and its metaphysic will reflect the relative importance it attaches to the different elements presented to it. It does not appear to be the duty of a particular science to decide such questions. The danger that has arisen from metaphysics in the past has depended upon the ease with which it is possible for metaphysical notions to influence empirical inquiries, and we shall find that it is possible to trace the influence of such assumptions at the present day operating unnoticed because they are entertained unconsciously, or their metaphysical character is not understood.

The truth is, of course, that it is impossible to begin work in a particular field without some pre-suppositions about its nature, and about how to set to work. The sciences arose on the basis of the preliminary work of this kind which had been done by pre-scientific thinkers, and is enshrined in common-sense. It is in connexion with methods of approach, methods of thinking and abstracting, that *demands* and hypotheses play such an important rôle. Some of these demands may be metaphysical in the sense that they lead us to make certain assumptions about the character of the whole field of study which cannot be put to a decisive empirical test. Instead of waiting to find out what that general character is after a good deal of work has been done, certain assumptions are made about it at the start. The history of science shows that progress has very largely depended upon the skill with which a few men of genius have grasped the kind of assumptions it was necessary to make. But these facts are by no means so widely understood as they should be, and consequently many people do not realise to what an extent such assumptions underlie scientific procedure, what their real character is, or how they have been arrived at. It is necessary that such buried assumptions should, from time to time, be dragged out into the light, in order to remind us how much our theoretical conclusions depend upon them, and to enable us to see whether they are still performing their proper functions and not leading us astray. Thus a science should be conscious of the assumptions and demands upon which it rests, but it seems that in natural science at the present day it is only physics—or rather its best representatives—that can be said in this way to be 'self-conscious'.

An important consequence of these facts is that a given set of assumptions will commit us to a certain circle of thought from which the only way of escape will be by overhauling the assumptions themselves. But when a particular way of thinking has got deeply embedded it becomes extraordinarily difficult to extricate oneself from it, and even if a man succeeds in overcoming the resistance of his own mind in this way tradition will still keep the general trend of thought in the same groove. When we hear of this or that institution creating obstacles to scientific progress we must also remember that scientific tradition may itself become an obstacle. 'The victory of the categories,' writes Professor Hobhouse, 'is not established without a struggle, and like other victories it ends in a dictatorship under which death or exile is the penalty of recalcitrance.' Thus a thinker who is not prepared to criticise his demands and assumptions or 'suffer them to be doubted of' is thought-bound. But to-day we are witnessing an attack upon the categories which is likely to have consequences as far reaching as the categories themselves are deep-seated, and we cannot meet criticism by merely turning a deaf ear. Nor shall we be able to benefit by the fruits of such criticism if we do not take the trouble necessary to understand them. It will be useful in what follows to distinguish two ways in which such fundamental notions may be entertained. For a given science they should always be adopted only as guides for investigation—not as solutions or conclusions—otherwise science would be deserting its proper function, and would merely be solving its problems 'metaphysically' in advance of empirical inquiry. Accordingly we shall say that for a given science all such assumptions are held *methodologically*, but where they are held to be true of reality then they will be said to be entertained *metaphysically*.

This distinction between the methodological and the metaphysical use of notions is an extremely important one and will play a considerable part in later discussions. But so often are these two points of view confused in biological controversy that it will be desirable to devote a little more attention to the meaning of the ambiguous term 'metaphysics'. In scientific literature it is not uncommon to find this term employed for *any* theory or notion which is at all out of the routine of common-sense, or traditional scientific, thought, and this has the unfortunate consequence of perpetuating the

belief that they are in some way eternally antagonistic to one another. Sometimes, indeed frequently, the term is used as though the adjective 'metaphysical' were in same way opposed to 'materialistic'. This is illustrated by the following passage:

> 'Here we must pass by the question, to be faced in a later chapter, how we can steer between a metaphysical Scylla and a materialistic Charybdis.'[1]

This introduces confusion into the very beginning because it sets a part in opposition to the whole since materialism is a *particular* kind of metaphysical theory in the sense described above. It has, it is true, become associated with natural science with special intimacy because natural science is concerned with what is called the material world. But a theory about the material world can be scientific without being materialistic in the sense in which that term is commonly understood, as is indeed the case with much of modern physical theory. The term metaphysics or metaphysical is applied by some authors to any theory which attempts to go beyond the immediate data of sense. This appears to be the meaning attached to the term by such writers as Ernst Mach and Karl Pearson whose views will be examined in detail in Part I. The expression, 'immediate data of sense', is ambiguous, but, in the sense in which these authors understand it, most if not all our scientific theories as commonly understood would be metaphysical—a fact which is not always clearly appreciated by many who quote their works with approval. Mach and Pearson hold that such theories are not to be held as true of reality but should be regarded as mere devices for imposing order upon sense-experience. But their view is itself based on a theory of the nature of sense-experience which is not very commonly held at the present day by philosophers.

Let us look now at some opinions of philosophers themselves about the meaning of the term 'metaphysics'. They, presumably, will know what they are talking about. The well-known remark of William James that 'Metaphysics means nothing but an unusually obstinate effort to think clearly'[2] gives emphasis to one aspect by which metaphysics is distinguished from science. It does not imply that 'clear thinking' is not required in science. The emphasis is on the

[1] P. Geddes and J. A. Thomson, *Biology*. London, 1915, p. 19.
[2] *Principles of Psychology*, London, 1890, Vol. I, p. 145.

'unusually obstinate' nature of the effort and refers to the fact already mentioned that metaphysics, in so far as it attempts to embrace the whole of experience cannot lighten its task by ignoring anything. Professor W. P. Montague writes that:

He then proceeds to distinguish two kinds of metaphysics—analytical and synthetic. He says that analytical metaphysics or Ontology is 'the study of the basic categories of the sciences'. This appears to be much the same as what Professor Whitehead means when he says:

> 'By "metaphysics" I mean the science which seeks to discover the general ideas which are indispensably relevant to the analysis of everything that happens.'[1]

Professor Montague applies the term synthetic metaphysics or Cosmology to 'the study of the generic conclusions of the sciences . . . which by the interrelating of these produces a unified picture of the world as a whole'. This last meaning of the term clearly marks off at least a possible science—a synthesis of knowledge professing to give us true information about the mutual relations of the various aspects of our experience. And it is in this sense that I have been using the term. The trouble is to make clear the distinctions between this and certain other sciences which are related to and apt to be confused with it. The principal difficulty for the modern world arises from the fact that it is believed that a prior question has to be faced before we can attain to such a universal science—quite apart from the question whether we have enough data for such an attempt. And this prior question relates to the nature and trustworthiness of knowledge itself. We are reminded at every turn, as soon as we begin to reflect, that it is the world *as known* that we are dealing with, and that the interpretation of the content of knowledge cannot be wholly divorced from the inquiry into the nature of knowledge. Now this latter inquiry—or epistemology as it is called at the present day—is included by some within metaphysics and is included, I think, in the above definitions of metaphysics by Professor Whitehead, and of analytical metaphysics by Professor Montague. It is because I wish to

[1] *The Ways of Knowing*, London, 1925, p. 31.
[2] *Religion in the Making*, Cambridge, 1926, p. 48.

keep these two points of view separate as far as is possible that I am anxious to confine the term metaphysics to the sense I have already explained. It is extremely difficult to convey what is meant in a few words at the outset, and I must rely on the indulgence of the reader to permit me to do so in the course of this Introduction and in Part I. Meanwhile an example will perhaps make things a little clearer. We hear a great deal to-day about different kinds of 'space'. We hear of Euclidean and non-Euclidean space. People also speak of visual and tactual space, as well as of physical space. Now the question of the existence and nature of physical space I should suppose to be a metaphysical one, but the problem whether visual space is to be called space at all and what its relation to physical space may be I should call an epistemological problem. And any particular notion of space employed in physical science for descriptive purposes, independently of its metaphysical interpretation, such a notion I should say is employed methodologically, i.e. simply for the purpose of investigation, which can be carried out quite independently of the difficulties raised by the more 'unusually obstinate effort to think clearly' which a metaphysical theory of space would require.

So much, then, for what I intend to mean by the word metaphysics, and for what I understand to be its relation to the special sciences. Another term which is apt to arise in connexion with discussions about the logic of science is 'philosophy'. Like the terms 'science' and 'metaphysics' this term also appears to be used very loosely and ambiguously. Sometimes it seems to be used as a synonym for metaphysics, and sometimes as a wider term which includes metaphysics as a part. It is most frequently employed to include a group of sciences having certain common characteristics as regards subject-matter and method and excluding the natural sciences. What, then, is the distinction between the 'natural' and the 'philosophical' sciences? We have noted that in the natural sciences the minds which pursue them are brought into relation with their subject-matters through sense-perception, and the distinction might be based upon this. Professor Whitehead, for example, says: 'Nature is

that which we observe in perception through the senses.'[1] If we adopt this as a provisional definition of 'nature' we can define the philosophical sciences negatively as those whose subject-matters are *not* (primarily) brought into relation with minds through sense-perception. Marked out in this way the following would then be some of the principal philosophical sciences: Logic (including pure mathematics), Ethics, Aesthetics, and Psychology. Whereas some of the principal natural sciences would be Physics, Chemistry, Geology and Biology.

There is however a difficulty about this division which arises over psychology and biology. In spite of the fact that we know organisms through sense and therefore study them all at bottom by the *same* methods, whereas we know minds primarily *not* through sense-perception—in spite of this fact it is believed that minds and organisms (or at least some of them) are closely related. And it does not seem desirable to cut the organism in two in this way and study the 'parts' separately in two widely different branches of science. This of course raises metaphysical and logical problems of great difficulty depending on just this fact that we are in the first instance brought into relation to the two subject-matters in different ways. But fortunately this problem will not arise for detailed consideration from the biological point of view until we get to the end of Part II, and it suffices meanwhile to point out how the difficulty is connected in this way with the duality of our modes of relation to different subject-matters. And there is no occasion to make any special mystery out of one mode of relation more than another, since, as we shall see, one is as 'intelligible' (or 'unintelligible') as another.

Neither is there any need to discuss further possible meanings of the term 'philosophy'. All I have attempted to do is to mention certain sciences sometimes called 'mental and moral' which are regarded as especially closely related to philosophy, and to suggest one way in which they differ from the group of natural sciences. At the present day many people would exclude psychology from this list on the grounds that it now has no closer relation to philosophy than the natural sciences. It is also distinguished, as we shall see presently, in another way from the remaining philosophical sciences mentioned.

[1] *The Concept of Nature*, Cambridge, 1926, p. 3.

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An inspection of the list of sciences given in the above section shows that the different sciences stand in very different relations to one another. For example, apart from metaphysical assumptions, physics and biology stand *alongside* one another as two sciences of nature (i.e. of 'that which we observe in perception through the senses'). But the relation of psychology and logic to one another and to the remaining sciences seems to be peculiar, and not describable as 'alongside.' For example, psychology has certain relations to all the remaining sciences depending on the fact that all the sciences owe their existence to certain types of mental activity, especially to the type of activity we have already alluded to as 'intellectual.'[1] But there are other types: the æsthetic, the moral, and the religious, and each activity has what we may call its 'outcome.' We have already referred to the outcome of the intellectual activity as 'knowledge.' Now if we go back to our list of sciences we see that they fall into three groups. The natural sciences deal with the processes of nature (in the sense defined); psychology studies mental processes of all kinds *as such*; whilst the remaining sciences mentioned—Logic, Aesthetics and Ethics, to which, presumably, we may add Theology, while depending on the existence of certain mental activities, are not concerned with those activities as such but with their *outcome*. Perhaps this relation of psychology and these 'sciences of outcome' to one another may be illustrated by the following analogy. The textile engineer is concerned with the working of textile machinery but not with the woven product. The textile designer, on the other hand, is not concerned with the working of the machinery but only with the woven product—the outcome of its working. But the designer cannot afford to be ignorant of the nature of textile machinery and of the technique of weaving because these will set certain limits to, and impose certain characteristics upon, his designs.

Now logic and psychology clearly have a special relation to all the remaining sciences. All the sciences, we have said, have this in common: they are all the outcome of intellectual operations directed upon some subject-matter or other. But psychology is the science which studies *all* mental processes

[1] See above, p. 24.

as such, and logic is the science which studies the outcome of the intellectual operations. It has to do with the investigation of propositions, their different types and properties, and with the validity of interferences from them. It seems, therefore, as though psychology and logic would bear somewhat the same relation to one another as the textile engineer bears to the textile designer. Logic, although only concerned with the outcome of the intellectual operations, will (at least in the widest sense of the term logic to include theory of knowledge) have to take the teaching of psychology into account in case it should be found that *other* mental processes than purely intellectual ones may interfere with the outcome of the latter. But logic, even in its more restricted sense, will be related to all the other sciences (including psychology) in another way. The sciences aim at making valid inferences about their subject-matters since they all want their propositions to be true. Consequently it would seem that the ultimate appeal of the other sciences will be to logic.

But when I say that logic is fundamental to the other sciences in this way I do not mean that a given science cannot be successfully pursued in complete ignorance of logic as a science, for this is palpably untrue. It would mean that knowledge of logic was prior to the exercise of the intellectual operations, which is absurd. Anyone who possesses the requisite intellectual gifts, anyone who is capable of performing the necessary mental operations, is able to pursue scientific inquiry without knowing anything about either the mental operations involved or the logical laws which govern their outcome, without, in fact, knowing what, from a logical point of view, he is doing: just as it is possible to perform athletic feats in complete ignorance of anatomy and physiology.

Nevertheless, some knowledge of such things might, on occasion, be helpful in avoiding pitfalls to which a too naïve view would expose the investigator, and still more the theorizer—just as some knowledge of optics is useful to the worker with the microscope, and some knowledge of physiology—dietetics and so forth—is useful to the athlete. We here come upon a distinction to which reference has already been made and which it is necessary to keep in mind. I refer to the difference between the outlook and the requirements of the investigator on the one hand, and the interpreter or 'organizer' on the other. Much discussion of scientific methodology

is, I think, apt to be vitiated from a failure to recognize this difference. Logic will be of little help to the investigator because here so much depends on the nature of the subject-matter and he alone will have the requisite first-hand acquaintance with it. He is guided not by conscious discursive processes but by 'tact', 'insight' or 'intuition' about which neither he nor the logician knows anything. Here logic will avail him but little. It is in the interpretation of the outcome of investigation that logic becomes of importance. We have seen that the interpretations of a particular science do not profess to be in any sense ultimate. They claim to be true only within the circle of assumptions and modes of abstraction considered by the investigator to be suitable within a given science. Now logic (in the wide sense in which I am using that term) provides a critique not only of the inferences by which a given interpretation is reached within a given circle of assumptions, but also of those assumptions themselves. Moreover there are still deeper critical questions to be asked as will be gathered from the following passage from J. S. Mill:

> 'Of the science, therefore, which expounds the operations of the human understanding in the pursuit of truth, one essential part is the inquiry: What are the facts which are the objects of intuition or consciousness, and what are those which we merely infer?'[1]

Mill adds that 'this question has never been considered a portion of logic,' but belongs to metaphysics. And so it does if by this question is meant what is the ontological 'nature' of what we are given in sense experience. But if the question means what is the significance of what is so given for *knowledge* then it is an epistemological problem,[2] and as such it belongs or is closely related to, logic in the wide sense. How we divide the problems and what we include in logic is not so important as to understand clearly what those problems are and how they are related to the procedure of science. Now clearly, this fundamental question: What are the facts which are the objects of consciousness? (which, for natural science, I interpret to mean: What exactly *do* we become aware of through the senses?) is one which is fundamental to all scien-

[1] *Logic*: Introduction, § 4. Eighth edit., London, 1925.

[2] My reasons for wishing to regard epistemology as having closer affinities with logic than with metaphysics (synthetic) will be clear I hope from what was said on p. 30. See also pp. 114, 131, 169 below.

tific theories, and, therefore, in this sense also logic with epistemology is fundamental to all the sciences.

Exponents of particular sciences may and do (especially in mathematical physics) from time to time undertake the investigation of such problems and the criticism of their own fundamental assumptions, but in so doing they are not pursuing their own science but epistemology. It is open to the investigator to make whatever assumptions, and to use whatever mode of abstraction he considers helpful—and in this he is usually the best judge so far as the progress of discovery is concerned—but the final test of the truth of such assumptions, and of any inferences that may be based upon them, rests with the science which makes the critical study of such things its special province.

This account of the mutual relations of the natural and the philosophical sciences would, I believe, be agreed to in principle by the majority of philosophers. But how far it would receive the assent of the followers of natural science seems more doubtful. To an extreme behaviourist it will presumably be meaningless since, if I understand him rightly, he would not acknowledge the existence of what I have referred to as 'intellectual activities'; and what I have called their outcome he would call, I suppose, laryngeal movements, or other bodily changes. Also it is a little difficult to see how what I have said is to be reconciled with the following assertion by a physiologist:

> 'The same law which determines the downward growth of the root in plants is responsible for the existence to-day of all the sciences of which mankind is proud.'[1]

If this is true it would seem that we ought, in order to understand the nature of knowledge, to consult not a logician or even a psychologist, but a botanist. These extreme opinions I should prefer to regard as specimens of natural science run wild into unconscious metaphysics. Generalizations of such wide compass as this appear to be too shaky in their own foundations to induce us to depart from orthodoxy in such matters.

We can pause here and survey the ground so far covered. I have suggested that the way to study the biological antitheses—form and function, preformation and epigenesis,

[1] E. H. Starling, *Principles of Human Physiology*, London, 1912, p. 6.

vitalism and mechanism—is by an investigation of biology as knowledge. I have suggested that this will involve a study of the nature of biological propositions and of the principles of systematization by which they are built into a science. I have also attempted briefly to make clear what I understand to be the relations of the natural sciences to other departments of knowledge, and particularly to logic. I have given reason for regarding epistemology as of special importance when we come to interpretation because it provides a critique of all those notions that are fundamental to scientific procedure. Also I have used the term epistemology to include Mill's fundamental question. One way of understanding the deep-seated assumptions of our scientific procedure is by learning how they came to be introduced in the development of science, and to this we now turn.

What I now wish to do is to discuss certain crucial events in the history of European thought which were responsible for stamping upon modern science some of its most characteristic features. These events have been the subject of much discussion in recent years but such discussions do not appear to be so familiar to biologists as to render reference to them superfluous.

Everyone knows that the ferment of thought which we call the Renaissance resulted, so far as science is concerned, in an overthrow of the Aristotelian physics and, at least in part, of the Aristotelian metaphysics, which had dominated western thought for many centuries. It is equally well known that the man who, perhaps more than any other, was responsible for this change was Galileo. But it is much less well known to what an extent deliberate discussion of what we now call epistemological problems preceded and accompanied these changes,[1] and to what an extent Galileo himself introduced certain assumptions of this kind which had the most far-reaching consequences.[2] This important aspect of the Renaissance is, unfortunately, not clearly brought out in histories of science, which deal solely with the experimental

[1] For an account of these early discussions see E. Cassirer, *Das Erkenntnisproblem*, 3te Aufl., Berlin, 1922, Bd. 1.

[2] His opponents urged against Galileo 'that he had devoted more years to the study of philosophy than months to the study of physics'.

side of Galileo's work. The philosophical basis of the changes he introduced is not discussed. Galileo is therefore best known as the man who performed the celebrated experiment from the campanile at Pisa, studied the pendulum, and introduced the important concept of acceleration—all of which were of fundamental importance in setting physical science on its triumphal path. But Galileo was responsible for changes in thought which have had reverberations far beyond the confines of physics, and it is these which I now wish to describe. There are seven features of modern science which may be said to have been introduced by Galileo, and when I say 'introduced' I do not mean that he was solely responsible for inventing them, because the germs of many of them can be found in Greek thought. The important point is that they were features of Greek thought which did not find favour with Aristotle, and consequently did not find a place in the Aristotelian tradition which was the dominant way of thinking in Galileo's time. These seven features are as follows:

(1) In the first place Galileo's whole procedure rested on the conviction that a thorough-going correspondence and harmony existed between mathematical truths and the occurrences in nature.

(2) As a consequence of this the Aristotelian teleological view of nature gave way to the concept of causal relations according to law as the groundwork of nature and the proper object of scientific study.

(3) With this view was also coupled the tendency to exclude from nature whatever elements it appeared to contain which were non-metrical and could not be treated mathematically.

The above three features might be called metaphysical or ontological in the sense that they are concerned with the nature of the *object* of scientific study. The remaining four are methodological in so far as they have to do with the *procedure* of physical science.

(4) Galileo does not appear to have concerned himself much with the ultimate grounds or *logical* justification of his views, so long as he was able to show their practical value as guides in investigation. He was perhaps the first to see that such justification was not necessary for empirical investigation, and was therefore the first to use the piece-meal method of science by restricting the scope of problems and dealing with one at a time.

(5) In accordance with the above a mathematical law was first thought out and entertained hypothetically until it was tested by experiment to determine whether it was upheld by experience.

(6) With Galileo began the departure from the Aristotelian method of searching after the 'inner nature' of things as substances with attributes, and a turning towards the study of the relations between things, leaving their nature undetermined. This feature—so characteristic of modern physics—is discussed in Galileo's letters on sun-spots. (See *Lettere intorno alle macchie solari*, Opera III, 462.)

(7) Connected with this is the demand that the outcome of an investigation should not bring the process to an end, i.e. explanations must not be 'final' or 'ultimate' but should leave problems for further study.[1]

Of these seven points the third is the one of most interest from the present point of view. It will be best illustrated by the following quotation from *Il Saggiatore* (Opera, IV, 333 ff.):

> 'I feel myself impelled by the necessity, as soon as I conceive a piece of matter or corporeal substance, of conceiving that in its own nature it is bounded and figured in such and such a figure, that in relation to others it is large or small, that it is in this or that place, in this or that time, that it is in motion or remains at rest, that it touches or does not touch another body, that it is single, few or many; in short by no imagination can a body be separated from such conditions: but that it must be white or red, bitter or sweet, sounding or mute, of a pleasant or unpleasant odour, I do not perceive my mind forced to acknowledge it necessarily accompanied by such conditions, so if my senses were not the escorts, perhaps the reason or the imagination by itself would never have arrived at them. Hence I think that these tastes, odours, colours, etc., on the side of the object in which they seem to exist, are nothing else than mere names, but hold their residence solely in the sensitive body; so that if the animal were removed, every such quality would be abolished and annihilated. Nevertheless, as soon as we have imposed names on them, particular and different from those of the primary and real accidents, we induce ourselves to believe that they also exist just as truly and really as the latter.'[2]

What thoughts and emotions this passage conjures up in the modern reader when he considers all the subsequent developments of the thought it contains! Here Galileo makes, on behalf of physical science, the celebrated distinction between

[1] We see the operation of this demand in the objections to 'vitalistic' explanations in biology at the present day.

[2] For this translation I am indebted to E. A. Burtt's *Metaphysical Foundations of Modern Physical Science* (Kegan Paul, London).

what later came to be called 'primary' and 'secondary' qualities. He saw that for the mathematical treatment of nature the secondary qualities—colour, smell, etc., could be ignored, and thus discovered an important *method of abstraction*. It was not necessary, however, from this standpoint, to cast aspersions on their 'reality'. In doing so Galileo went beyond strictly methodological requirements, but this has been the traditional attitude of physics since his time. The secondary qualities have been the orphans of the world of scientific thought. Nobody wants them but nobody can get away from them. The belief that they are 'unreal' has been a cardinal point in all materialistic metaphysics, because they constitute an obstacle to all neat and tidy explanations of the world in terms of bits of stuff pushing each other about. But this, at least until recently, has been regarded as the type of scientific explanation *par excellence*, and its metaphysical success (as distinct from the methodological) has been partly dependent upon the ease with which the secondary qualities can be forgotten in natural science. This is one of the ways in which natural science, if it is not on its guard, may unwittingly slip into metaphysics. It is but a particular instance of the general fact that when abstraction is made of anything the latter does not thereby cease to exist but returns to claim a place in the scheme later on when, after successful analysis, we turn to the question of synthesis. For the purposes of mathematical physics it is not necessary to have any theory about secondary qualities. All that is requisite is that it should in fact be possible to neglect them in formulating certain quantitative laws. But a metaphysical theory, as we have seen (p. 23), cannot abstract, cannot ignore anything, and is therefore driven to have some theory regarding the secondary qualities. Consequently the theory that the secondary qualities are 'unreal', which is a metaphysical theory as far as mathematical physics is concerned, must be called upon to justify itself at the bar of critical philosophy, and explain what it means by 'unreal'. The existence of the secondary qualities is, as Mill remarked,[1] the great obstacle to universal generalizations of the mathematico-physical type: a point which might with advantage be explained more clearly and conscientiously to students of natural science.

In histories of science so much emphasis is usually laid

[1] *System of Logic*, Bk. III, ch. xiv, 2.

on Galileo's troubles with the Church that the really important logical principles involved are obscured. What Galileo was challenging was a well-entrenched system of thought. It happened unfortunately that the doctrines of the Church had developed in relation to this system and had found in it a philosophical basis. Consequently to attack the one was to attack the other, or so at least it was believed. Now the point of most importance is that Galileo and his successors succeeded in altering to a very large extent the way of thinking in general of the educated world. It was not merely a question of new facts being discovered leading to old theories being abandoned. A totally new attitude towards existence, new ways of asking and answering questions, new fundamental notions about both thought and things, were set on foot by men who probably saw only a little way in the direction in which such changes would lead.

An imaginary example from recent times will make my meaning more clear. Within the present century startling changes have been set going in our ways of thinking, and for these changes mathematical physicists have again chiefly been responsible. There have been searching investigations in logic and into its relations to pure mathematics, and the most far-reaching changes in our ideas about space and time are in progress. Now suppose that the existing churches were in some way concerned with preserving the Newtonian interpretation of the laws of motion. Einstein and his followers would then be heretics in the eyes of the churches, just as were those who attacked the Aristotelian philosophy in the sixteenth century. But now, as then, this would be quite incidental to the real philosophical problems at issue, and a man might take one side or the other irrespective of his relation to the Church. The opposition to the new views of Galileo was not based simply on religious motives as is so commonly supposed. Similarly we find at the present day that some scientific men oppose the new ways of thinking that are now being offered—perhaps because they have been trained to the older notions which have been taken for 'absolute' and unalterable.[1] To adopt new ideas involves a mental upheaval and a reorientation which is as difficult for us to-day as it was in the time of Galileo. It is a mistake to emphasize the reli-

[1] See, for example, the remarks of Prof. Rignano, *Biological Memory*. London, 1926, p. 225.

glons at the expense of the logical and psychological opposition to Galileo's innovations. What I wish to suggest, therefore, is that to understand the Renaissance we have to regard it as a replacement of one way of thinking—held to be unalterable—by another, and further that to-day we are entering upon a new Renaissance which is likely to be as revolutionary and far-reaching in its consequences as the first, since it too involves a totally different way of regarding nature from that which came into being as a result of the Renaissance of the time of Galileo. Moreover, all the signs indicate that biology will play a more important part in the thought of the future than it has done in that of the past. But it will not be fitted to do this if it is not prepared to undertake an overhaul of its basal principles to an extent that has scarcely yet been considered—since those principles are themselves based on the system of thought which is in process of transformation.

I do not wish to give the impression that in an intellectual revolution of the kind we have been considering nothing remains of the older system which the new one displaces. For example: whereas the Aristotelian physics and metaphysics were so widely departed from by Galileo, his logic (and part of its metaphysical background) remained and has only been effectively criticized and extended in quite recent years. Galileo himself uses the notions of substance and accident in the passage quoted on p. 39. Moreover, there may have been elements in the Aristotelian system which were valuable but which were rejected by the enthusiastic supporters of the new doctrines because no place could be found for them. In the changes in progress to-day it is not so much a question of completely abandoning older notions but of reinterpreting them, and it may be that our new interpretations will attempt to embrace factors which the older ones were content to ignore. So soon as the belief in its absoluteness is destroyed, so soon will the cement which kept together the parts of the old system be loosened, and a freer and less rigid way of thinking will become more generally possible, with beneficial results, it may be, for biological science. Just as formerly it was the existing philosophy which obstructed the development of new ideas, so now it is the inertia of thought itself which becomes an obstacle to the unfolding of new ways of scientific thinking. But to-day the churches have no interest in obstruction and the issues are correspondingly clearer.

When the progress of science is spoken of we must bear in mind the two aspects of discovery and interpretation. Progress in the one by no means implies progress in the other. While we can unquestionably speak of serial progress in the case of discovery, progress in interpretation has by no means been a simple serial affair, but a process better described as 'oscillatory'. This is clear from a moment's consideration of some of the traditional alternative explanations which can be found in any branch of science but particularly in biology —now one and now another being in favour, without any decisive result being reached, and no ground being so sure as to be held as absolute and not liable some day to revision. If we run over our stock of fundamental explanatory notions we find that most of them have been 'anticipated' speculatively in Greek thought. What I wish to suggest, therefore, is that we may be as mistaken as were the peripatetic philosophers of the Middle Ages if we suppose that we are in possession of infallible principles of interpretation.

Before passing on to the further developments of Galileo's innovations a few illustrations will be useful of the way in which a well-entrenched system of thought is able to blind the investigator. A good example of this is seen in the history of the Ptolemaic system of the heavens. Ptolemy's interpretation of the movements of the heavenly bodies seems to have been overshadowed by the metaphysical notion of 'perfection' which had been handed down in the teaching of Aristotle. The motions of the heavenly bodies must, it was held, be perfect, and, as the most perfect figure was believed to be the circle, it was asserted that the motions of these bodies were in circles without more ado.

> "It is almost tragic to think that Ptolemy made his teaching to fit this theory, for there is no doubt that with all the ability and originality displayed by him in his writings, the history of astronomy would have made very different reading but for the influence of Aristotle on his premises."[1]

This will serve as an elementary example of the kind of 'blinding' influence which a metaphysical notion may have on new investigations and interpretations. But we must remember that Ptolemy was doubtless brought up in the doctrines of his time, and built his theory upon them without seriously calling them in question. Why should he? His

[1] J. B. Hart, *Makers of Science*, Oxford, 1923, p. 47.

theory on those foundations fitted the facts as they were then known quite well, except in the case of the planets, and here he devised the theory of 'deferents' quite in the scientific manner to meet their special peculiarities. Even when, many years later in 1542, the Copernican system was published, it was by no means opposed solely by the Church.

> 'While various astronomers of some eminence that gave support to the Copernican system, almost from the beginning, it unfortunately chanced that by far the most famous of the immediate successors of Copernicus declined to accept the theory of the earth's motion. This was Tycho Brahe, one of the greatest observing astronomers of any age. . . . It should be added, however, that he accepted that part of the Copernican theory which makes the sun the centre of all the planetary motions, the earth being excepted. He thus developed a system of his own, which was in some sort a compromise between the Ptolemaic and the Copernican systems. . . . This combined scheme, it should be added, *may be made to explain the observed motions of the heavenly bodies*, but it involves a much more complex mechanism than is postulated by the Copernican theory.'[1]

I have italicized the sentence in the above passage which is of special interest in the present discussion. It shows that it was not *only* the 'adverse testimony of the Hebrew prophet' which led Tycho Brahe to decline the doctrine of Copernicus. He had a theory of his own *which also fitted the facts*. We are dealing therefore, in this case, not simply with a conflict between science and metaphysics, but between two rival scientific hypotheses. One had religious tradition behind it and the other had greater *simplicity*, and it was the latter that finally triumphed. In this choice between the simpler of two rival theories we see the germs of a characteristic feature of modern science. These instances also illustrate another point which is not always sufficiently emphasized in histories of science, the fact namely, that views and principles which seem obvious, even 'axiomatic', to us, were by no means so to a previous age. Historians of science are too apt to give the impression that we are necessarily nearer the truth than our predecessors, and are too much inclined to depict science as having at last, after many struggles and possibly wrong turnings, reached resting places of certainty. Whereas another reading of history would suggest that our theories may present the same aspect to the historian of the future as those of the sixteenth century do to us, and that he also will

[1] H. S. Williams, *History of Science*, Vol. II, p. 64.

speculate about the 'influences' which blinded us. But what might those influences be? Is it possible that some of the tentative ideas which have been stifled and buried in the past might have deserved a better fate? Or have the requisite ideas for the systematization of biological knowledge not yet even been conceived, owing, perhaps, to the very success of existing ones in aiding the accumulation of facts? But: 'The growth of a science,' says Professor Whitehead, 'is not primarily in bulk, but in ideas.'[1] Accordingly the 'blinding influence' may be a too great reliance upon facts at the expense of ideas, or, in other words, due to the lack of a 'closer and purer alliance' between the 'experimental and the rational faculties' of which Bacon speaks in the passage already quoted on p. 26. Dr. C. D. Broad, writing about the way in which the natural sciences develop, says:

> 'They flounder about in the dark till some man of genius sees what are the really fundamental factors and the really fundamental structure of the region of phenomena under investigation. In mechanics the keystone is the notion of acceleration; in chemistry it is the theory of elements and compounds and the conservation of mass; in economics, perhaps, it is the notion of marginal utility. Sciences where no such discovery has yet been made, such, e.g., as psychology and biology are almost at a prescientific level; their inductions carry no great conviction to anyone trained in the more advanced sciences.'[2]

A good example of the influence of a metaphysical theory on biological thought is furnished by the way in which the doctrines of Leibniz and of Malebranche, especially the former, were responsible for the persistence of the belief in preformation throughout the eighteenth century and long after sufficient empirical data had been accumulated to show its untenability. And if any further instances are necessary to enforce the contention that existing scientific beliefs may constitute a bar to scientific progress, it is only necessary to recall the treatment of Pasteur at the hands of the Parisian physicians, of John Newlands by the chemists of his time, and of Young and Ohm by their contemporaries in physical science. After this digression it is necessary to return to the difficult problems which were implicit in the innovations introduced by Galileo into European thought.

[1] *Introduction to Mathematics*, p. 115.

[2] *The Relation between Induction and Probability*, II: *Mind*, N.S. XXIX, p. 45, 1920.

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Biology has familiarized us with the notion of development as a process of differentiation. Whatever can be said to develop shows the same process of splitting into parts with specialized functions, whether it be an organism or a human institution. We find that the same is true of the development of thought, and Galileo may be said to have taken the first step towards the differentiation of the methodological or scientific point of view from the metaphysical. But that this process of differentiation is still incomplete and is still not understood is clear from the following passage :

> ' Energy is the underlying cause of all changes in matter. This does not seem a very satisfactory definition but, so far, it is the only one possible. It is a very striking fact that the two fundamentals of our external world, matter and energy, have for us no existence apart from their effect on us. We cannot prove that there are such things except in so far as they manifest themselves, matter by being changed, and energy by producing changes, which in turn alter our sensation-complex.'[1]

There is an almost mediaeval flavour about the first part of this passage although it is from a recent book undertaking to instruct the biological investigator in the principles of physics. The author fails to see what Galileo saw so clearly : that it is quite unnecessary to say what matter *is*, or what energy *is*, in order to investigate the goings on in the physical world. The last sentence of the above passage, on the other hand, is decidedly post-Galilean and, to an unsophisticated reader must seem strangely discordant with the first. He will wonder how it comes about that, if all that ' has existence for us ' is our ' sensation-complex ', we should require something other than this to ' produce changes ' in it, and why, given that, something else still should be required to ' produce changes ' in *it*. It would seem simpler to have only one ' unknowable ' and this is in fact the position to which modern physics is tending. The above passage thus bears upon its face, mingled in a curious way, traces from the whole sweep of thought from the Aristotelians to Bishop Berkeley, and,

[1] D. Berry, *An Introduction to Biophysics*. London, 1921, p. 3. Contrast this with the following from Poincaré (*Science and Hypothesis*. Eng. trans. p. 98) : ' When we say force is the cause of motion we are talking metaphysics ; and this definition, if we had to be content with it, would be absolutely fruitless, would lead to absolutely nothing. For a definition to be of any use it must tell us how to measure force . . . it is by no means necessary to know what force is in itself.'

between the mediæval and the modern attitudes towards the *metaphysical* issues involved, the *scientific* standpoint which Galileo grasped so clearly escapes altogether. It therefore illustrates the failure to differentiate between the scientific and the metaphysical points of view, and also furnishes an example of what M. Meyerson means when he says :

> ' L'homme fait de la métaphysique comme il respire, sans le vouloir et surtout sans s'en douter la plupart du temps.'[1]

But from another point of view the passage I have quoted illustrates the consequences of differentiation without compensating co-ordination. It illustrates, that is to say, the manner in which the pursuit of science has become divorced from the critical study of scientific concepts and procedure. How these two studies came to be divorced it will be the task of this and the following sections to explain.

Galileo was not a systematic philosopher. He did not undertake to give a logical justification of his new method. He was content to explore nature with the aid of his new methodological discovery. But it was not long before a new metaphysics arose which was destined not only to oust the Aristotelian doctrines but also to play the same dominating rôle over modern thought as that of Aristotle had done over the Middle Ages. Thus no sooner had thought been set free from one fixed form and become fluid again than it began to crystallize into another. If Galileo was the first modern ' scientist ', Descartes was perhaps the last philosopher of the kind typified by Aristotle—one who had a first-hand acquaintance with the whole knowledge of his time, was responsible for creating a great part of it himself, and also constructed a complete metaphysic on the basis of it. The point to be emphasized here is that he not only set a number of sciences on their feet, but also set thought into a new groove, or, to be accurate, into two new grooves. Descartes was born in 1596 when Galileo had reached the age of thirty-two. He accepted and successfully extended Galileo's methodological principles, but he was not content with methodology. He was a rationalist and wanted to justify his methodology by rational principles. Now the only way to understand a philosophical writer is by reading his own works, not by reading what other

[1] Emile Meyerson, *De l'explication dans les sciences*, Paris, 1921, Tome I. p. 6.

people say about him. Consequently in what follows I do not profess to give such an account of the Cartesian doctrines as will relieve the reader from the labour of consulting the original, still less one which would satisfy an historian of philosophy. All I require to do is to depict enough of the philosophical ancestry of our modern scientific notions to enable us to understand them better. This seems all the more necessary in the case of Descartes in view of the misunderstandings about him which one meets in biological literature. Dendy, for example, wrote:

> 'More than three centuries ago the philosopher Descartes endeavoured to explain the human body as a machine, but as a machine under the control of the "soul," which he currently located in that part of the brain known as the pineal gland. His ideas of physiology, however, were, naturally, of the crudest description, and immense strides have been made in this direction since his "soul" of Descartes' philosophy corresponds [illegible] force" of some more recent

Two comments should be made on this passage: First, that it reveals a complete misunderstanding of the significance of the soul in the Cartesian doctrines, since the latter was concerned with knowledge, not with physiological details; and secondly, Dendy fails to understand that Descartes' physiology of the nervous system has served as the foundation for all that has since been done in the interpretation of that system, and the modern view has *in principle* departed but little from the lead which Descartes gave to it. Anyone can satisfy himself of this from a perusal of Huxley's essay on animal automatism, or, better still, of Descartes' own treatise *De l'Homme*.

Another type of misunderstanding, and one which is particularly germane to the present discussion, is revealed in the following passage:

> " . . . Descartes was an armchair philosopher, and his strong advocacy and practice of experimentation had a profound influence in directing men to "*la nouvelle methode*."'[2]

[1] Dendy, *Outlines of Evolutionary Biology*, London, 1912, p. 11. The following passage reveals a still more serious misunderstanding of Descartes, because it not only rests on a failure to comprehend the epistemological significance of the soul in his system, but implies that he was guilty of the worst form of intellectual dishonesty: 'It is true that Descartes endowed the human body with a soul: but it is probable that he did this to avoid trouble with the ecclesiastical authorities.'—A. D. Darbishire, *An Introduction to a Biology*, London, 1917, p. 86.

[2] Sir William Osler, *Evolution of Modern Medicine*, 1923, p. 174.

All this is perfectly true but Descartes, in addition to being a laboratory philosopher, was *also* an armchair philosopher. (Nay, he was a *bad* philosopher, for he devised his co-ordinate geometry in bed !) It is with Descartes in his capacity as an armchair philosopher that we are here especially concerned, because his influence was as great in this capacity (even on medicine) as in the other, and all the more subtle because it is less openly recognized and understood. I have already given reasons for believing that it is silly to set up armchair and laboratory philosophy against one another as two unconnected and opposed modes of inquiry. If our purpose is to think an arm chair is as good a place to do it in as a laboratory (perhaps better) and Descartes was addicted to the practice of thinking as well as experimenting. Moreover, the way people think to-day, whether in laboratories or elsewhere, has largely been determined by the way Descartes thought some three centuries ago. That is why his thoughts are of such importance to us to-day if our aim is to learn to think independently. But: 'Operations of thought are like cavalry charges in a battle—they are strictly limited in number, they require fresh horses, and must only be made at decisive moments.'[1] That is one reason why people prefer to go on using Descartes' thoughts, rather than seek fresh ones.

Like all the great thinkers Descartes begins with an expression of dissatisfaction with the teaching current at the time. In all that he had been taught in the schools he found so much disagreement that he resolved to reject all that was taught merely by example and custom and search for a method of thinking things out for himself, for in his travels he had come to the belief that a man of good sense, using his own unprejudiced judgment, could come nearer to the truth than by the aid of the sciences contained in books. He accordingly drew up for himself four rules which may be concisely stated as follows: First, to accept nothing not clearly known to be true; secondly, to divide each problem into parts; thirdly, to proceed from the simple to the more complex; and fourthly, to aim at the greatest possible completeness and generality. Finding that of all the knowledge he had gained the mathematical was the most certain in its demonstrations on account of its dealing only with the most general relations and proportions between objects he resolved

[1] A. N. Whitehead, *Introduction to Mathematics*, London, p. 61.

to follow the mathematical method. But he saw that the other sciences depended upon other principles and in consequence it was first necessary to establish these. How was it possible for the mathematical notions, such as the geometrical point and line, which are never found in the physical world, to be applied to that world? If the secondary qualities do not belong to the physical world, and if the senses are so deceitful, how could we have any reliable knowledge of that world through sense, especially when we find we have the same sort of experience in dreams as we do in waking life?

In his search for an absolutely certain basis for knowledge Descartes instituted the celebrated method of doubt. He writes: 'I thought I ought to reject as absolutely false all opinions in regard to which I could suppose the least ground for doubt, in order to ascertain whether after that there remained aught in my belief that was wholly indubitable.' But where was a basis free from doubt to be found? Not in the objects that the senses present to us, because the latter are notoriously deceitful, nor in reasonings, even in geometry, because men sometimes err in reasoning. He found he could even doubt the existence of his own body, but not that while he thought he was something. He therefore concluded that he was certainly a thinking being—'a substance whose whole essence or nature consists only in thinking, and which, that it may exist, has no need of place, nor is dependent on any material thing.' Thus Descartes made the individual consciousness prior to all else in knowledge and certainty, a step which was to have consequences of the utmost importance for later science as well as for philosophy. The reader will easily detect a trace of this in the last sentence of the passage from Mr. Burns's book quoted on page 46. It is clear that for Descartes the mind or soul had nothing to do with the physiological processes, as entelechies are supposed to have, but was simply that in man which thought, and was more clearly and certainly known than the body. As regards the body Descartes made no distinction between man and the animals—both alike were machines, but animals did not think and had no minds or souls. This is clear from the following passage:

> 'And here I specially stayed to show that, were there such machines exactly resembling in organs and outward form an ape or any other irrational animal, we could have no means of knowing that they were in any respect of a different nature from these

> ——is; but if there were machines bearing the image of our bodies and capable of imitating our actions as far as it is morally possible, there would still remain two most certain tests whereby to know that they were not therefore really men. Of these the first is that they could never use words or other signs arranged in such a manner as is competent to us in order to declare our thoughts to others: for we may easily conceive a machine to be so constructed that it emits vocables, and even that it emits some correspondent to the action upon it of external objects which cause a change in its organs; for example, if touched in a particular place it may demand what we wish to say to it; if in another it may cry out that it is hurt, and such like; but not that it should arrange them variously so as appositely to reply to what is said in its presence, as men of the lowest grade of intellect can do. The second test is, that although such machines might execute many things with equal or perhaps greater perfection than any of us, they would, without doubt, fail in certain others from which it could be discovered that they did not act from knowledge but solely from the disposition of their organs: for while reason is a universal instrument that is alike available on every occasion, these organs, on the contrary, need a particular arrangement for each particular action; whence it must be morally impossible that there should exist in any machine a diversity of organs sufficient to enable it to act in all the occurrences of life, in any way in which our reason enables us to act. Again by means of these tests we may likewise know the difference between men and beasts. For it is highly deserving of remark, that there are no men so dull and stupid, not even idiots, as to be incapable of joining together different words, and thereby constructing a declaration by which to make their thoughts understood; and that on the other hand, there is no other animal, however perfect or happily circumstanced, which can do the like."[1]

There is no occasion here to stop to consider how such arguments stand at the present day. Huxley, accepting Descartes' view of the automatism of animals, and arguing from 'continuity', concluded that men also were automatons but *conscious* automatons. The point of importance for the present discussion is that Descartes believed that knowledge of this consciousness was prior to, and more certain than, that of the body, and he further concluded that it could exist independently of the body. Now this did not make it any easier to see how it was possible to have trustworthy knowledge of the physical world. The way Descartes attempted to ensure this was somewhat as follows: He found himself possessed of the idea of a perfect being, of a being more perfect than himself, since to doubt was an imperfection. He identified this perfect being with the God of theology, and on His goodness he rested the certainty of our knowledge of the physical world, arguing that God would not have given us a

[1] *Discourse on Method*, Part V.

belief in the physical world if this belief was illusory, because that would imply that God was deceitful and this would be incompatible with His goodness and perfection. The difficulty of the deceitfulness of the senses was overcome by the doctrine that the senses were merely subservient to bodily life and not for knowledge. Knowledge has to do only with 'clear and distinct ideas' and we only have such ideas of mathematical entities. The problem of the application of such ideas to the physical world was dealt with in the following way. Descartes not only denied physical reality to the secondary qualities as Galileo had done, but he whittled down the 'real' attributes of physical things to a still more drastic extent. He held that such qualities as shape, motion, weight, etc., were not essential attributes of physical things because they were all variable. Only one feature remained which could not be thought away, namely, three-dimensional spatial extension. Consequently he came to identify space or extension with the 'substance' of the physical world, whether it was 'empty' to the sense or not. Moreover this was known not by sense but by an intellectual act of the mind in which resided the (for Descartes) innate ideas of pure mathematics.

> 'But, in conclusion, I find I have insensibly reverted to the point I desired; for, since it is now manifest to me that bodies themselves are not properly perceived by the sense nor by the faculty of imagination, but by the intellect alone, and since they are not perceived because they are seen and touched, but only because they are understood, I readily discover that there is nothing more easily or clearly apprehended than my own mind.'[1]

A word more must be said about 'substance' especially on account of the important criticisms of this notion which are in progress at the present day. By 'substance' Aristotle had meant, primarily, an individual thing, e.g. an individual man was a substance with essential attributes, e.g. 'two-leggedness' and accidental attributes, e.g. 'whiteness'. But Descartes meant by a substance that which was capable of existence independently of anything else. Strictly speaking only God was a substance in this sense, but Descartes extended the notion to (1) that which had the essential attribute of spatial extension, and (2) that which had the essential attribute of thought. The only connexion between these two substances according to Descartes was through the pineal body of the human brain.

[1] *Meditations on the First Philosophy*, II.

This very brief and crude sketch of the Cartesian philosophy will, I hope, suffice to indicate the origin of two doctrines which, on account of their subsequent influence, are of fundamental importance for understanding the perplexities which, from the standpoint of biological science, I shall attempt to unravel. These two doctrines are: (1) the doctrine of the priority and certainty of the knowledge of self, and (2) the Cartesian dualism of thinking substance and extended substance. I must pass over Descartes' treatment of causation and how he came to neglect time, as well as the way in which he attempted to account for the existence of differentiations in the one extended but continuous substance. But there is one very interesting passage towards the end of the *Principles of Philosophy*, Section XVII, which I should like to quote. It deals with 'the things which our senses do not perceive'.

> 'But here some one will perhaps repl[y], that although I have supposed causes which could produce [natural objects, we ought not on this account to conclude that t[hey] were produced by these causes; for, just as the same artisan [can] make two clocks, which, though they both equally will indi[cate] the time, and are not different in outward appearance, [nev]ertheless nothing resembling in the composition of the [wheels, so doubtless the Supreme Maker of things has an infinit[y] of diverse means at his disposal, by each of which he could ha[ve] made all the things of this world to appear as we see them, without it being possible for the human mind to know which of all [these means he chose to employ. I most freely concede [and] I believe that I have done all that was required if it [is] that their effects accurately correspond [with] the phenomena of nature, without determining wi[th] that they are actually produced.

The biological reader will have no difficulty in appreciating the appositeness of these remarks to some of our biological theories.

The immediate consequences of the Cartesian dualism now require consideration. The world being cut into two utterly disparate parts in this way it was not surprising that from the time of Descartes onwards a corresponding cleavage of thought and investigation should also take place. Philosophy in the old sense now became cleft into two branches which soon parted company and, from henceforth, were pursued by investigators who tended more and more to have very little intercourse with one another. Science or *natural* philosophy concentrated its attention on exploiting the Galilean methodology, taking no further interest in the problem about the

nature of knowledge which Descartes had raised. Its attitude became purely 'objective'. Believing itself to be in contact with reality the other side of the relation of knowing (the knower) ceased to be a topic of scientific interest. And when I speak here of science I understand the physical sciences and the biological sciences which modelled themselves on the physical; psychology as an empirical science as we know it to-day had not yet received its birth from the mother of sciences. Philosophy, i.e. what remained after 'natural philosophy' had been separated from philosophy in the all-embracing sense of Descartes, was thus left with all the difficulties which the experimentalists found it convenient to discard. In this way the differentiation of thought was set going in earnest, but whereas in an organism the differentiating process is accompanied by integration of the parts, the whole still remaining a unity, in the differentiation of thought the two parts fell asunder and remained without influence on one another. Instead of pooling their results and relating their interpretations to existence as a whole the natural sciences developed a metaphysic of their own. They dealt with Descartes' extended substance and their metaphysic was materialism. The philosophical sciences dealt with the thinking substance and their metaphysic was idealism or 'mentalism'. From the standpoint of materialism mind was a purely superfluous epiphenomenon. From the standpoint of mentalism the existence of anything which was not mental came to be regarded as so speculative and uncertain as to be meaningless for human knowledge. But such extremes would never have been reached if the cleavage of the two lines of thought had not been so complete, because, as a moment's consideration will show, it is with *knowledge* of the material world that the natural sciences are dealing, and the interpretation of knowledge requires reference to Descartes' 'thinking principle'. But materialism is primarily a doctrine about the nature of the physical world, not about knowledge. It holds that what people call matter is the primary stuff of existence, and that what people call mind is some sort of a secondary bi-product of matter.[1] People do not come to such a point of view by reflecting on the nature of knowledge but by reflecting on the discoveries of natural science which

[1] Not that mind does not *exist* as Prof. Goodrich seems to suppose. See his *Living Organisms*, Oxford, 1924, p. 174.

abstracts from minds, and from much else, as we have seen, and it is a perfectly intelligible outcome of such reflexions on the basis of such abstractions. But when the materialist comes to consider knowledge he finds it difficult to find a place for it, or indeed to see how it is possible at all. Philosophical idealism on the other hand arose from inquiries into the nature of knowledge. It had its origin in reflexions on the problems bequeathed to us by Descartes. Thus the examination of knowledge led to quite opposite results from the reflexions on the discoveries of science about nature. If from different starting points we come to quite contradictory conclusions it would seem that either one starting point is wrong, or that both are in some way defective, and we have to find some way of reconciling them. The followers of science at first took no heed of these contradictions, but in the nineteenth century these philosophical criticisms began to make themselves felt and led to the abandonment of materialism among those men of science who considered such things, and to its replacement by the doctrine of phenomenalism.[1] How far this change was satisfactory and what its consequences have been we shall have to inquire shortly. Materialism is obviously at a disadvantage because it has not first answered the questions: How do we come by our knowledge? What reliance can be put upon it? and, What sort of questions is it competent to answer? The materialist cannot escape from these questions because it is upon knowledge of nature that his own conclusions are based, and, if he comes to conclusions which make knowledge either impossible or meaningless he undermines his own foundations, and his labours have been in vain. It seems clear that if we are to establish the conclusions of any science we must have firm foundations for human knowledge, and if our science reaches results which make knowledge unreliable there must clearly be some erroneous inferences present or otherwise that science involves itself in the general ruin. In other words: a scientific doctrine which makes nonsense of human knowledge cannot itself be true because it involves itself in a vicious circle; and on the other hand, a theory of knowledge which makes nonsense of science must, if it is true, show how it is that we come to possess the large amount of reliable scientific knowledge we do in fact appear to possess. All this seems so obvious that it is hardly to be expected that

[1] The sense in which this term is used is explained in Part I.

it could be forgotten but so far is this from being the case that we frequently find examples of both types of mistake.

It will have been noticed that Descartes' inquiries about knowledge included Mill's fundamental question:

> 'What are the facts which are the objects of intuition or consciousness, and what are those which we merely infer?'

Before passing to an examination of the answer to this question which is offered by phenomenalism (which appears to be a common attitude among men of science at the present day) I should like to offer some more examples of the consequences of the bifurcation of thought which followed the Cartesian dualism. One general consequence which appears to me to be of interest to biologists is that, as a result of this cleavage and of the rapid development of physical science, only two fundamental ways of thinking have been exploited: the physical on the one hand, and the psychological on the other. Thus biology has had the misfortune to fall between two stools. It has tended to look for aid in one direction or the other and an independent *biological* way of thinking has hardly been seriously contemplated. Had Galileo been attracted to biological problems biology might to-day be in a very different position and physics might have been less favourably situated.

But so heavily have the dice been loaded in favour of the physical way of looking at things that even the psychological point of view has not received the attention which perhaps it deserves. One of the most striking general features of modern biological science is its neglect of *thought* and of the mental side of life generally. One sees this exemplified in so many ways. It is hardly necessary to point out the almost universal prevalence of the belief that what are called mechanical explanations are the only *possible* ones for biological progress.[1] The common opinion is expressed by Professor E. B. Wilson when he writes: "It is our scientific habit of thought to regard the operation of any scientific system as determined primarily by its specific physico-chemical composition." In another place he says we accept this hypothesis because 'it has proved itself useful in discovery and has kept us moving in the right direction'. No reason is given for the prevalent view. It modestly calls itself a 'habit of mind'

[1] It is not suggested that we have to look to *psychology* for an alternative.

which is a good habit because it is heuristically useful. This illustrates Professor Whitehead's meaning when he says that science has remained 'predominantly an anti-rationalistic movement, based upon a naïve faith', and that it has 'never cared to justify its faith or to explain its meanings'. And he adds that there is 'a Nemesis which awaits upon those who deliberately avoid avenues of knowledge'. 'What is the sense', asks Professor Whitehead in another place, 'of talking about a mechanical explanation when you do not know what you mean by mechanics?'[1] It is to be hoped that when a mathematical physicist of Whitehead's eminence says things of this kind biologists will begin to take notice. They have always meekly followed the lead of physics even although it has not always been very fresh physics. But it is probably safe to assume in this case that if Professor Whitehead does not know what we mean by mechanics few living biologists do. Professor Wilson seems to be a little doubtful of himself when he says: 'we cannot hope to comprehend the activities of the living cell by analysis merely of its chemical composition, or even of its molecular structure alone.'

We shall, of course, examine these difficulties in earnest later; I mention them here simply by way of illustration of the common opinion. Then consider the prevalence of such doctrines, especially among physiologists, as the 'conscious automaton' theory, or the doctrine of psycho-physical parallelism. What are they, from the biological standpoint, but devices for shouldering away the psychical implications of living things, in order to preserve the old exclusively physical point of view? Not, of course, that those who hold these doctrines do so in any but the best of faith. They feel compelled by the 'facts' to refuse any other attitude, when in truth it may be merely that the demands and assumptions on which physical science has been built leave no loophole for any other point of view, and it may be that we have to do simply with a case of 'blindness' following from failure to distinguish a methodological procedure from metaphysical

And lest any one should suppose that these matters are of 'purely academic interest' let me call attention to their immense practical consequences in only one field, that,

[1] A. N. Whitehead, *Science and the Modern World*, Cambridge, 1927, p. 18.

namely, of medicine. Here the physical point of view has been, and still is acting, the part of a dead weight of prejudice against a psychological study of mental disease, both in practice and in the medical schools. The old metaphysical assumptions of materialism render men blind to the obvious fact that you can learn more about a man's mind by talking to him for ten minutes than by looking at sections of his brain for ten years, (not to mention the fact that whatever information may be obtained by the latter method will be of little avail for the treatment of the patient!) And yet thousands of men have devoted their lives to the belief that the *only* path of progress in this department of medicine is to be found in the histology of the brain, and in such physiological information as can be gleaned in this and other similar ways. It is almost impossible to get a hearing for any other view and in the average medical school the student has little opportunity of learning any other. New theories are covered with abuse and irrelevant misrepresentation, but are rarely sympathetically and intelligently criticized.

We see a further instance of what I have called the neglect of thought in scientific education. As a general rule the courses of instruction in the biological sciences in our universities seem to consist of little more than dogmatic teaching of the opinions current at the time, from the standpoint of the particular teacher, with little attempt to encourage the student to take a critical point of view of his own. Science is taught as something fixed and stable, not as a system of knowledge whose life and value depends on its mobility and readiness to revise at a moment's notice its most cherished constructions when new facts or a new point of view suggest the possibility of alternative ones. In the morphological branches especially the student is called upon to exercise little more than his memory. It is these facts at their worst which tend rightly to discredit science as an instrument of education. Much could be done by way of improvement by expecting more than mere learning in the examination, by giving due critical attention to the theoretical parts of the subject and to the historical and logical foundations of its thought. Professor H. E. Armstrong has voiced some of these opinions in the following words:

> 'To-day physical doctrine is laid down as something absolute—not as tentative. No cleric was ever more absolute than is the

> modern outside physicist.[1] *Gasman's* is not in it with a school text-book of chemistry. 'There is no sitting down before facts—facts are just used as brick-bats for the poor boy to catch and throw back if he can. . . . Medicine to-day is in far worse plight than it was then (in Huxley's days)—the burden of facts laid upon the students is ever increasing, and one that it is impossible for them to bear. . . . Scientific method is neither used nor taught in the early stages of the medical student's career: he is eternally crammed into stupidity.'[2]

We shall find ourselves later on confronted from time to time with the question of the aim and purpose of science. It will therefore be desirable to consider this question now with a view to discovering what attitude towards it should be adopted. It is sometimes said that 'the primary object of science is to reduce the course of events to laws of uniform sequence, and so to facilitate prediction, and an interference with the course of nature for the specific purposes of man'.[3] And when the question is raised whether men are justified in devoting their lives to science instead of adding their quota to the mundane affairs of life the answer is usually given in the affirmative on the ground that the products of scientific investigation sooner or later reflect upon the mundane pursuits. If the medical sciences are in question it is usually urged that they have for their object the alleviation of human suffering. And in general the pursuit of science is regarded as resulting in benefit to humanity, and that is its aim and justification. There are, however, two difficulties in the way of accepting this simple interpretation. On the one hand only a relatively small proportion of scientific knowledge has, so far at least, found any application; and, on the other hand, many of the applications can hardly be called beneficial at all. These two difficulties must be examined in more detail. After that we can try to discover some other alternative.

The physical sciences have left their mark on daily life to a greater extent than any others. Medical science comes second, and may be considered roughly under the three heads of surgery, medicine and hygiene. Surgery is based on

[1] This is not true of the more philosophical modern physicists, but it is hardly an exaggeration of the average school text-book, and of the way natural science is taught in schools. [J.H.W.]

[2] H. E. Armstrong, *Huxley's Message to Education*, Nature, May 9th, 1925.

[3] F. H. A. Marshall, Mind, N.S., Vol. XXIX, 1920, p. 67.

anatomy, medicine on physiology, and hygiene on parasitology. These three together involve a very considerable part of biological knowledge, especially if we include those branches which have some connection with anatomy, physiology and parasitology in the narrower sense. These again find a further application in agriculture. It must however be remembered that none of the so-called pure sciences were originally established and pursued with any intention of their leading to such applications. They were carried on solely for the satisfaction they brought to the pursuer, so that at that time they can hardly have found their justification in practical application. In illustration of this one need only recall the labours of the early minute anatomists in the eighteenth century, who delighted in dissecting and describing the details of insect organization, etc. Even to-day a great deal of scientific work can hardly claim any such justification. Consider, for example, the four or five theories of the origin of the vertebrates, or the speculations regarding the mutual affinities of the various coelomate phyla. Now, however, that so many apparently 'useless' investigations have later been found to have important applications (e.g. classification of mosquitos) it is common to justify such researches on the ground that sooner or later applications may be found for them and that therefore no province can be neglected. But in spite of this we still find those who pride themselves on being 'practical men' condemning this or that branch of study as useless. Nor is this confined to critics outside science. We find exponents of one branch condemning another branch of science as exhausted and useless. Perhaps the most extraordinary of all criticisms of this sort are those that have from time to time been directed against pure mathematics—surely a clear case of killing the goose that lays the golden eggs. Again and again mathematical inquiries of the most abstract kind pursued solely for their intrinsic interest without any thought of ever finding any application have later received brilliant examplification in physics. Of this we have recently had a remarkable example in the use made by Einstein of the investigations of Gauss and Riemann. And yet practical men are always to be found who deprecate such studies as fanciful and meaningless. 'But they were wrong, as such men always are when they desert their proper function of masticating food which others have prepared'.[1]

But quite apart from mathematical and scientific pursuits there are others, such as art and philosophy, which no one supposes to have practical application however much they may indirectly influence human life. If the pursuit of science is only to be justified by the hope of finding application what standing have philosophy and art? These, like science, are pursued simply 'for their own sake'—often with little or no encouragement or remuneration, and the same has often been the case with science. It is true that in modern civilized countries these pursuits are taught in institutions and the teachers receive a salary, but even so the pay is not supposed to bear the same relation to the work done as it does in the pursuits related to commerce and the ordinary callings of life. Money does, however, play a much greater part in modern science than it formerly did on account of the cost of the elaborate apparatus needed for modern investigations. This state of affairs puts the man of science into an embarrassing position. Formerly he was free to pursue whatever line of inquiry his genius and interest suggested. If now he accepts money for equipment he is in danger of being expected to show 'results'. And what sort of results? If he himself preaches the gospel of utility to the admiring public how can he expect them to appreciate any other aspect of his work? They will interpret his appeals as meaning: Give money for research in order that you may be more comfortable; not—in order that you may have knowledge because knowledge is in and for itself good. Thus comfort is made the goal of civilization because comfort is usually the only goal that the people who supply the money can understand, and this is the inevitable consequence if men of science represent the primary goal of science as prediction in the interests of utility.[1]

[1] Cf. the following from Zimmern, *The Greek Commonwealth*, Oxford, 1915, pp. 312-3.

'We think of the Greeks as the pioneers of civilization, and unconsciously credit them with the material blessings and comforts in which we moderns have been taught and are trying to teach Asiatics and Africans, to think that civilization consists. We forget that they were more innocent of most of these than the up-country Greeks of to-day, or than most Englishmen were before the Industrial Revolution. We must learn how to be civilized without being comfortable. Or rather we must learn to enjoy the society of people for whom comfort meant something very different from motor-cars and arm-chairs, who, although or because they lived plainly and austerely and sat at the table of life without expecting any dessert, were aware of the use and beauty and goodness of the few things which were vouchsafed them—

This brings us to the second difficulty in the way of a utilitarian justification for science—the difficulty, namely, that some of the applications of science are not beneficial to humanity. If science claims credit for and finds its justification in, its beneficial applications, it must clearly also accept responsibility for those that are harmful. It must accept responsibility for both or for neither. Now it is very difficult to find any general agreement of opinion about what is beneficial and what is not. There will not be much difference of opinion about such inventions as submarines, poison-gas, and high-explosive projectiles. Individual nations who first used these instruments of modern barbarism may have derived a temporary benefit to themselves, but when they became generally adopted the only result seems to be that the whole civilised world becomes committed to maintaining them—a vivid example of the enslavement of mankind by machines, because he lacks either the will or the wit to free himself from their toils. Then there are those mechanical contrivances whose use is not confined to war—industrial machinery, methods of rapid transport, and cheap and easy sources of amusement, such as cinemas and 'wireless'. It is extremely difficult to assess these things because they all have their positive and negative aspects. The difference between a civilization with them and one without them is much the same as the difference between a mouse and an amoeba. They both do much the same sort of things. But one does them slowly and clumsily and the other does them quickly and precisely thanks to the possession of better tools. Can we find any application which is wholly beneficial? It might be expected that medical science would furnish such an example, but even here objections have been raised. Some eugenists object that our medical science keeps alive the physically and mentally unfit. Similarly birth control is hailed by some as a blessing and by some as a curse.

In the face of such conflicting opinions the man of science is apt to shrug his shoulders and deny all responsibility for this state of affairs, but in that case he cannot pick out those

their minds, their bodies, and Nature outside and around them. Greek literature, like the Gospels, "is a great protest against the modern view that the really important thing is to be comfortable. The comfort promised by the Gospels" (and that enjoyed by the Greeks, whether the same or something different) "and the comfort assured by modern inventions and appliances are as different as ideals can be."[1]

applications which are commonly applauded and claim credit for them. If he is not responsible for the harmful applications or misapplications, neither is he for the useful ones, and if this is admitted he cannot base his pursuits on their utilitarian aspect. He cannot do this for the two major reasons I have given—first, the fact that much scientific investigation is pursued solely for non-utilitarian ends, and secondly because there is no general agreement in regard to the blessings of a large part of those discoveries that have found a practical application.

It is not difficult to discern some of the elements that have contributed towards maintaining the common utilitarian view of the aim of science. People are in too big a hurry to 'explain' everything, and too ready to accept uncritically the simple explanations offered by naturalistic metaphysics. They approach these questions only from the genetic point of view without any regard to possible limitations in that method.[1] If man is found to exhibit any peculiarity not shared by the lower animals it is orthodox to believe that this *must* be an animal activity in a more complicated or otherwise disguised form, and natural selection is invoked to explain its 'origin' and persistence. Consequently everything must be shown to be useful or be damned. Thus we find a physiologist saying:

> 'It is the same property of adaptation, the deciding factor in the struggle for existence, which impels the greatest thinkers of our time to spend long years of toil in the invention of means for the offence and defence of their community, or for the protection of mankind against disease and death.'[2]

But we have seen that even if this is true at all it is only true for an extremely restricted circle of intellectual activity. Are we seriously asked to believe that Milton writing *Paradise Lost* or the Greek mathematicians investigating conic sections, or even Darwin collecting data for the *Origin of Species* were simply 'doing their bit' in the 'struggle for existence'? The truth is that this is not a question for natural science at all and consequently we cannot expect an answer to it from specialists in natural science. Moreover the doctrine of natural selection was originally put forward as a scientific hypothesis—not as a universal metaphysical principle. It

[1] See below, p. 395
[2] Starling, *Principles of Human Physiology*, p. 6.

is still the subject of debate even among biologists—quite apart from any difficulties which logical criticism may have to point out. Consequently it is a little premature to use it as a metaphysical principle for the 'explanation' of everything under the sun. It is extraordinary how this and other scientific theories have been mixed up with questions with which they really have nothing to do. Thus Loeb writes:

> 'Darwin's work has been compared to that of Copernicus and Galileo inasmuch as all these men freed the mind from the results of Aristotelian philosophy which, with the efficient co-operation of the church and the predatory system of economics, caused the stagnation, immorality, and misery of the Middle Ages.'[1]

It may reasonably be doubted whether there is such a simple correlation between the theories which these three men contributed to science, on the one hand, and economics, morality and social welfare on the other, as this passage, and the following one from the Preface of the same book, seem to suggest. Loeb explains that the book is dedicated to d'Alembert, Diderot, Holbach and Voltaire because they were the first to follow the consequences of a mechanistic science to the 'rules of human conduct' and thereby:

> 'laid the foundation of that spirit of tolerance, justice, and gentleness which was the hope of our civilization until it was buried under the wave of homicidal emotion which has swept through the world.'

Are we really required to believe that the theories of Copernicus and Galileo, with the co-operation of the men in the above list, transformed Europeans from being the victims of stagnation, immorality and misery into paragons of all the virtues until, for some reason which is not given, they relapsed into barbarism in 1914? Were there no wars, no misery, no immorality, no predatory economics during the eighteenth and nineteenth centuries? How, then, shall we describe the Industrial Revolution? Did the spirit of tolerance, justice, and gentleness walk through the land in those days? Is not a moment's reflexion sufficient to show the absurdity of these wild generalizations from natural science into ethics and sociology?

Apart from all speculations about their origin and metaphysical interpretation we distinguished, in a previous section, four fundamental human activities: the intellectual, the

[1] J. Loeb, *The Organism as a Whole*, London, 1916, p. 348.

aesthetic, the moral, and the religious. Each appears to be an activity *sui generis*—no one being reducible to another. Each, we said, has its 'outcome', and the outcome of the intellectual activity is knowledge. To each activity there corresponds one of the fundamental values—that related to the intellectual activity being called truth. The pursuit of science and philosophy, the two modes of the intellectual activity, depends on the fact that some men have an intuitive apprehension of the value of truth in and for itself, not because knowledge is useful. And not because any reason can be given why it should be so. We here touch upon that most perplexing problem—the relation between faith and reason. The exercise of the intellectual activity in the pursuit of science rests upon beliefs for which no reason can be given—belief in our intuitions of an external world, of other selves, and of the validity of inductive inference, as well as on the intuitive belief in truth and moral values. William James wrote:

> 'Science herself consults her heart when she lays it down that the infinite ascertainment of fact and correction of false belief are the supreme goods for man. Challenge the statement, and science can only repeat it oracularly, or else prove it by showing that such ascertainment and correction bring man all sorts of other goods which man's heart in turn declares.'[1]

That the whole pursuit of science rests upon moral judgments is clear from the fact that a man of science who falsifies his results does not break a logical law but an ethical one. Thus the various activities although separate are entwined in a curious way. The search for order, simplicity, and economy of expression are characteristics shared by both the intellectual and the aesthetic activities. Also we find mathematicians talking of the 'almost divine beauty' of certain equations.

If, therefore, natural science goes beyond its own sphere and refuses to acknowledge any other values but biological survival it undermines its own foundations and runs into theories which are plainly contradicted; as indeed always happens when reason, chafing at the necessity of taking something on trust, turns its critical analysis upon itself and finds that its own product dissolves away in utter scepticism. A harmony of the activities is the most difficult thing to attain because each is apt to be pursued in disregard of the others.

[1] *The Will to Believe*, London, 1897, p. 22. (Also in *Everyman's Lib.*, p. 117).

and in a given individual one is usually uppermost. Thus aesthetic values are sacrificed to economic ones when the erection of petrol pumps is allowed to desecrate the countryside without effective protest by a civilization brought up in the gospel of utility. The objections to vivisection, the criticisms of pure mathematics, and the conflict between natural science and religion are all instances of disharmonies between the four fundamental activities, and of the differences between the judgments of value of different men. When the conflict between science and religion is discussed sufficient care is not always taken to distinguish between conflicts between a scientific theory and a theological doctrine on the one hand, which is a logical conflict resting ultimately on the law of contradiction, and a conflict between science as a manifestation of the intellectual activity and religion, on the other hand, which is quite a different conflict and not simply a logical one. Conflicts of the latter kind can only be overcome by patience and toleration and by viewing them from a philosophical standpoint, and not—as sometimes happens—by means of speculative theories which make nonsense of one side or the other, or both.

I conclude, then, that the primary aim of a given science is to discover the truth about its particular subject-matter, and that it need have no other justification than the intuitive apprehension of men that knowledge, and the pursuit of which it is the outcome, are good. At the same time the results of scientific inquiry sometimes have applications to other aspects of human life, and some of these applications have, and are likely to continue to have, a profound influence upon it. But the responsibility for controlling these applications does not rest with science but with civilization at large. This control involves moral and political problems of extreme importance and urgency, and it is perhaps part of the duty of men of science as men to bring the seriousness of this part of their work to the notice of politicians and educators. But if men of science continue to propagate the belief that the primary aim and justification of their pursuits is utility they will be undermining their own surest foundations and doing a serious disservice to civilization as well. Because utility, unlike truth, is not an end in itself but a means to other ends which in the ultimate analysis reduce to physical comfort and pleasure, i.e. to biological not human values. Civiliza-

tions are judged by the things which they believe to be valuable, and a civilization which succeeds in so ordering its affairs that its members are sufficiently set free from the pressure of biological necessities to be able to cultivate the human values in harmony with one another, is said to be higher than one in which such freedom has not been reached, or in which it is merely employed for the intenser gratification of biological impulses.

It has been necessary to discuss this at some length for the following reason. All through the history of science its devotees have been inspired by some such faith in the value of truth as I have tried to describe. The Victorians were never tired of insisting that their primary aim was the unprejudiced pursuit of truth. But towards the end of the last century doubts began to be raised among men of science on the basis of doubts raised much earlier by philosophers. These doubts were based upon certain sceptical conclusions regarding human knowledge in general and hence they concern our present inquiry. They had their origin, in part at least, in Galileo's removal of the secondary qualities from nature. They frequently take the form of a denial of the possibility of attaching any meaning to truth but a purely practical one, and so lead inevitably to a utilitarian view of science. The upholders of this view among men of science are usually *phenomenalists*. They hold that science is not and cannot be concerned with reality as an external world of things, but solely with the supposed internal world of consciousness, and that the aim of science is simply to discover means of so ordering what appears to us in consciousness as to effect an economy of thought. It is of the utmost importance to examine this doctrine from the standpoint of its consequences for biology. This will be our first task in Part I. But there is still one more topic requiring a preliminary discussion in this Introduction.

A great deal of the discussion which goes on in theoretical biology is devoted to the relative merits of various types of *explanation*. But one elementary and fundamental point never seems to receive adequate attention: the disputants seldom endeavour to state clearly what they mean by, or

what they hope to attain by, explanation in general. Such causal expressions of opinion as we do find scattered about in scientific literature will not as a rule bear the briefest critical examination. When so little care is devoted to such a truly foundational question it is not surprising if much of the discussion which rests ultimately upon it should resolve itself into aimless beating of the air and what is called 'arguing at cross purposes.' In this section I propose to examine briefly some common notions on this question, and then to take a *prima facie* view of what appears to be involved in explanation. This will bring to light many of the root difficulties which obstruct our biological thinking because, for various reasons, they are obscured. In the sphere of morals it is said, I believe, that the first step to salvation is the conviction of sin. I think we might say that in the realm of the intellect the primary requisite of progress is a lively realisation of ignorance. And in order to persuade people to listen to a new point of view it is first necessary to show them that all is not well with the old. It is because biologists so seldom undertake a radical overhaul of their fundamental notions that old traditions are permitted to outlive their usefulness.

We may begin by examining the opinions of some biologists on the question of explanation. Professor E. S. Goodrich, on p. 21 of his *Living Organisms*, writes: 'Science advances by explaining, that is describing, the unknown in terms of the known.' This, doubtless, is not intended for a formal statement, but it is one which is very commonly encountered in scientific literature. Taken as it stands it clearly is meaningless: the unknown can hardly be dealt with at all, much less described, until it *is* known. Moreover, if *description* is all that is involved, why should we distinguish between, say, descriptive and 'causal' explanations in embryology? Clearly the author does not mean what he says. We can only guess what he does mean, and our guesses may be wrong. Perhaps all that is meant by 'known' and 'unknown' is 'familiar' and 'unfamiliar.' If this is so it would seem to suggest either that we are born into the world with a stock of 'familiars' with which we proceed to 'describe' the host of unfamiliars that press upon us, or that we devote our early days to the collection of such a stock of them as may be deemed sufficient for the explanation of the rest of experience. On either alternative the advance of science would appear to be a

somewhat haphazard and precarious business. Perhaps it is.

The following is a more detailed expression of opinion on this subject by Professor E. W. MacBride:

> "In what after all, does explanation consist? I think that close reflection on this subject will convince one that we think we have "explained" a new phenomenon when we have successfully compared it with some older phenomenon which we regard as familiar and well known. Thus we imagine that we have "explained" the eruption of a volcano when we have compared it rightly or wrongly, to the explosion of an overheated boiler, and the law of gravitation which explains the movements of the heavenly bodies is merely a comparison of these movements with the movements of an apple which falls from its parent tree to the earth. The explanation of development by an entelechy is at bottom a comparison of the forces moulding an embryo to the purposeful endeavours of a man who is bent on building a particular house of a particular type and who takes whatever materials he can lay his hands on in order to effect his object. Now certainly purposeful endeavour is the most familiar of all the activities which we see around us, and there is therefore nothing wrong in Driesch selecting this most familiar of all phenomena in order to throw light on the development of the germ. The great objection to it is, I think, that it is unfruitful: it does not enable us to compare one kind of development with another. For we simply have to instal a different kind of entelechy with a different purpose in every kind of egg and there the matter ends. On the other hand, there are records of phenomena, rapidly increasing in number with the extension of research, of which we can only give a rational account by postulating some form of the hypothesis of organ forming substances; for in some cases these substances are actually visible to the naked eye in the living egg."[1]

According to Professor MacBride, then, explanation consists in *successfully comparing new* phenomena with *older* and *more familiar* ones—suggesting that the familiar does not need to be explained. How we are to decide whether the comparison has been carried out 'rightly or wrongly' Professor MacBride does not say, but the criterion of success seems to be that the comparison should enable us to compare one phenomenon of the kind to be explained with another of the same kind. Now how do Professor MacBride's own examples and the actual procedure of science agree with this account of the matter? Are overheated boilers, and organ-forming substances really familiar and well known compared with developing eggs and volcanoes? Would a geologist be content with such an explanation of a volcano as is here offered? Might he not object that one had only to instal a

[1] *Presidential Address*, Zoo. Sect., British Association, 1914.

different boiler in each volcano and there the matter would end? Would this enable us to compare one volcano with another? No doubt such an explanation would suffice for common-sense purposes, but everyone will feel that there is something wrong with this account of the matter from the point of view of science.[1] Perhaps we shall get some insight if we consider a little more closely what Professor MacBride says about his organ forming substances. First he speaks of them as being hypothetical and as having to be 'postulated', and then as being, in some cases, 'actually visible to the naked eye in the living egg.' Now a thing cannot be both hypothetical and not hypothetical. Either the visible substances are organ forming (whatever that may mean) substances, in which case they are only hypothetical in those eggs in which no such thing is visible, or, they are not organ forming substances, and in that case they are not involved in the question but are hypothetical throughout. When we look into the matter we find that in most cases such visible materials can be centrifuged away to one pole of the egg without disturbing the course of development. On the other hand there are eggs in which it can apparently be shown that certain regions are essential for the development of certain organs. Clearly, then, in the first case nothing is gained by calling the visible materials 'organ forming substances' and in the latter case all that is meant is that certain materials are essential in the sense that bricks or similar materials are essential for house building, but not that they account for the particular *form* taken by the embryo, any more than bricks account for the *form* of a house—since all houses of brick are not of the same form. Thus the explanatory significance of organ forming substances is not at all clear. What usually happens when explanatory entities are 'postulated' in science is that hypothetical entities are constructed in thought which, if they exist, would enable us to understand how something happens. And the value of such procedure depends, if it is successful, on the way in which it enables generalisations to be made beyond the particular case and also deductions to be made which lead to the discovery of

[1] The reference to gravitation is equally unfortunate. The story of the apple is an utter travesty of Newton's procedure. It is said to have originated from Voltaire, and might now be allowed to sink into oblivion, so far as serious scientific literature is concerned.

unexpected cases. When this occurs the hypothesis is said to be 'verified', which does not mean that it is true. But this is different from Professor MacBride's account of the matter: the entity postulated, far from being 'familiar and well known' may be something quite new, as, for example, was the case with accessory food factors or vitamins. The essential requisite is not familiarity but that the entity postulated should have properties which will admit of verifiable deductions being made.[1] That is the decisive factor by which the relative merits of 'organ forming substances' and 'entelechies' if they are offered as scientific explanatory entities, are to be judged. This aspect of scientific explanation will be further illustrated by the following quotation from Professor A. S. Eddington:

> 'We believe that the ordinary objects of experience are very complex; in order to understand their mutual relations and to explain the phenomena, they may be resolved into simpler elements. Whilst it is a reasonable procedure to explain the complex in terms of the simple, this necessarily involves the paradox of explaining the familiar in terms of the unfamiliar. Thus the ultimate concepts of physics are of a nature which must be left undefined; we may describe how they behave but we cannot state what they *are* in any terms with which the mind is acquainted.'[2]

This statement clearly involves, at least in the case of modern physics, a total inversion of the view of explanation we have just discussed. The present state of affairs might more correctly be described as explaining the known in terms of the unknown—the paradox depending upon the ambiguity of the word 'known'. As we shall see later 'familiarity' is not involved at all in spite of the popular belief.

We shall now proceed to consider what is done in explanation from the standpoint of what Dr. Broad would perhaps call 'enlightened common sense' in order to see how far it will take us. Any difficulties that may arise will then be dealt with in Part I, and we shall return to this subject from the special standpoint of biology in Part II. Etymologically, the word means to make plain, just as the allied word 'ex-

[1] Perhaps a word of warning should be uttered regarding a too reckless use of the method of postulating. Mr. Bertrand Russell writes: 'The method of "postulating" what we want has many advantages; they are the same as the advantages of theft over honest toil.' (*Intro. to Mathematical Philosophy*, London, 1920, p. 71)

[2] *The Meaning of Matter and the Laws of Nature according to the Theory of Relativity*. Mind, N.S., XXIX, 145.

plicate' means to smooth out folds—to make what is obscure clear, or what is complex simple—all with reference, of course, to the human understanding. Now what methods are open to us for making the obscure more clear? There would seem to be two principal alternatives: either we can investigate that which is to be explained further in the hope of finding that what appeared complex contains simpler elements, or, we can seek some means of bringing it into relation with the rest of our knowledge. In pursuing the first method we might, of course, also achieve the second, but even if we were not so fortunate we should have accomplished something in the direction of explanation. The second alternative will always involve two things. (I intentionally use the vague expression 'thing' at this stage to cover both 'fact' and 'law'). On the one hand stands the thing to be explained, which need not by any means be 'new and unfamiliar,' and, on the other, stands the rest of our knowledge. The function of explanation is to bring them into 'harmony' as we say, but what do we mean by this expression? The thing to be explained may simply be a piece of sheer hard matter of fact which refuses to 'fit in' with the rest of our knowledge, or it may be a so-called 'empirical law' which refuses to have anything to do with any other law. We commonly speak of not being able to 'see any connexion' between the two things. Explaining or harmonizing in this case consists, then, of discovering or supposing some connexion or *relation* between the two. What then, are those relations which thus subserve the function of bringing those odds and ends of observed facts or sequences into harmony with the rest of knowledge? As a beginning we might take Hume's list of seven 'philosophical relations': identity, relations of space and time, quantity or number, degrees of quality, contrariety, and causation. The explanatory employment of each one of these might be discussed in detail but it will suffice to consider three which are of obvious importance: resemblance, number, and causation. Identity, as M. Meyerson points out, represents the ideal of the more fully developed sciences. Bosanquet quotes a story from Thackeray in illustration of the exercise of the notion of identity: 'An old abbé, talking among a party of intimate friends, happened to say, "A priest has strange experiences; why, ladies, my first penitent was a murderer." Upon this the principal

nobleman of the neighbourhood enters the room, "Ah, Abbé, here you are; do you know, ladies, I was the Abbé's first penitent, and I promise you my confession astonished him!"'

Relations of space and time, and particularly the significance of the modern views of these notions for biology will be discussed in Parts I and II.

(1) Resemblance. Resemblance plays a great part in the explanations of every-day life. Any common feature which enables us to assimilate the new to the old will suffice to docket our daily experience. But resemblance also plays an important part in the classificatory stages of science, and here of course 'degrees of quality' have their place. Thus we speak of explaining the taxonomic position of an animal or plant when we point out those features of resemblance to others which give it its place in the classificatory system. There is another use of resemblance in biological explanation: all those ingenious devices by which organic phenomena, such as amoeboid movement, are imitated by droplets of various inorganic mixtures, present us, by resemblance, with an intellectual bridge between the organism and the inorganic world.

(2) Number. In cases where units can be assigned in some way to what is to be explained, it may be possible to establish numerical relations between things, either with or without other types of explanation in conjunction with them. This also, of course, involves the notion of identity, since this method, if it is to be successful, requires that the units can be treated as if they were only *numerically* different. If the numerical relations can in any way be generalized inductively this method gives us the power of precise prevision. In this section it will be convenient to distinguish this method of explanation as *calculation*. It can only be employed when the entities involved have extensive magnitude, or when they are countable.

(3) Causation. This is commonly regarded as the explanatory instrument *par excellence* of science. Here again there are different degrees of precision and synthesis on different levels of thought. Common sense is content with assigning causes to effects without regard to the further relations between causes themselves. A spot of light dancing on the wall is sufficiently explained when it is found to be caused by sunlight reflected from a cup of tea on the breakfast

table, but science seeks to assimilate its causes to one another. Mill wrote: 'Explaining in the scientific sense, means resolving an uniformity which is not a law of causation into the laws of causation from which it results, or a complex law of causation into simpler and more general ones from which it is capable of being deductively inferred.' How this account of the matter stands at the present day, and what exactly we mean by causation in biology, will be considered in Part I. For the present we may understand the terms cause and effect in their most popular and homely sense. The rest of this section will be devoted to a more detailed consideration of an imaginary example of explanatory procedure and some of its consequences, and a little must first be said about what occasions the desire for explanation.

In daily life it is the unusual and unfamiliar which attracts attention and demands explanation, and this in turn is usually satisfied by any sort of relation to the familiar. The behaviour of a man standing on his head in the street is explained when we learn that he is doing it for a wager, or for other peoples' amusement, or that he is insane. Science also starts with the excitation of interest, but it does not make the familiar things of daily life its basis of reference, but rather its starting point. The man of science is distinguished by his ability to be mystified by occurrences which excite no wonder in every-day life, and, moreover, by his persistent refusal to continue to be mystified by such things. Action at a distance, the growth and development of animals and plants, which constitute such perennial problems for science, present no difficulties to the agricultural labourer. He deals with them daily, they form the very warp and woof of his existence and excite no wonder. In the man of science likewise the 'problem of knowledge' which is such a puzzle to philosophers excites no interest. Knowledge is the stuff in which he trades, and the intellect the principal tool he uses, but they are not the objects of his curiosity. And if it be true that science starts in this way with the familiar, its explanations can hardly be in terms of the familiar, except in the early classificatory stages when some familiar things are shown to be special cases of other more general familiar things. It is clear that as science advances it must get further away from the familiar, and therefore from the 'intelligible' in this sense of that term, until it creates a new world of familiars for itself, as is

indeed the case with the world of atoms and molecules. And herein lies a danger for science: we may grow too satisfied with our explanations when, through long and fruitful use, they become so familiar that we are lulled into forgetting that they conceal still further problems. Another and still commoner danger lies in forgetting how such explanations have been reached and so coming to regard explanatory entities as 'more real' than that which they were originally designed to explain. The loss of the capacity for being mystified is a serious one and this is perhaps why we sometimes feel disgust at a too complacent and cock-sure presentation of a scientific theory. We can now turn to the imaginary example.

Suppose we are sitting in a quiet room and see a chair suddenly begin to move across the floor. This is the kind of event which clamours for immediate explanation, for the obvious reason that chairs do not commonly behave in such a way. If we are not too much paralysed with fear our first impulse may be to investigate the *cause* of the movement. There are plenty of apparently uncaused movements going on about us which excite neither fear nor interest, but the movements of chairs do not belong to this list. Something we say must move it, and fear is quenched and curiosity satisfied by the discovery, let us say, of a black thread attached to the chair and some friend concealed in the room who is pulling it. Common sense would probably demand no more, but there are two principal ways in which a less easily satisfied curiosity might proceed to continue the investigation further.

(1) We might employ the calculating method—asking, for example, how much pull must be given to the thread to yield a given amount of movement in the chair. This method leads us to the laws of mechanics giving us confidence arising out of predictability and in that special sense we may be said to understand the process better. But clearly the intellect demands more than the ability to predict the possible movements of the chair, and such an achievement leaves hosts of further questions unanswered. We may therefore proceed in another way.

(2) We may discriminate different parts in the whole process, and ask *what* exactly happens in each part, and between the parts of the whole. For example: What happens between the chair and the floor when movement occurs, in the thread when it is stretched, in the man's body

when he pulls, or in the man's mind prior to the event? In doing this we have merely made a preliminary analysis of the whole process and assigned the parts to the sciences of mechanics, physiology and anatomy, and psychology. We might now try to apply method (1) to the results of such an analysis—the latter having supplied this method with new problems. We should thus be extending the calculating process and the power of prevision in so far as regular sequences of events admitting of generalization could be discovered, and in this special sense we should be extending the explanatory process. But there would be difficulties in extending this method very far to what happens in the man's body, and, apparently, no possibility of doing so at all in the case of his mind.

Anatomical investigation reveals an immense complexity of further parts, the mutual relations between which call for explanation. We may be said to have explained the grasping of the thread when we have shown what muscles are involved and how they act upon the bones. But just as the original movement of the chair demanded a cause, so we ask what *makes* the muscles contract. The discovery of the nerves and of their elementary properties presents an explanation in causal terms, although hardly in terms of a familiar cause. But the further pursuit of analysis still carries us on. We see that the nervous impulse offers no explanation of what happens in the muscle itself, but here we begin to reach the limits of perceptual analysis. Exactly the same thing happens in the physical analysis of the chair, the floor, and the thread, and here it might be supposed that the process would come to an end. But, as everyone knows, we are indebted to a few men of genius for the discovery of a means of continuing the analytical method further. But before we turn to this we must briefly consider what happens when the analysis is turned to the study of what happened in the man's mind prior to the event.

Here we once more make use of the relation of cause and effect for explanatory purposes. In daily life and in courts of law motives are investigated with a view to the discovery of the causes of human actions. In the present case an inquiry into the character of the man who pulled the thread would perhaps reveal the motive which lead to his behaviour. It was perhaps done for a joke to frighten the occupant of

the room, or perhaps as a more serious experiment to see how he would 'react', and so on. Some people are sufficiently interested in these things to inquire more systematically. They pursue the same methods of observation, analysis, classification and generalization as are employed in the other sciences, but when we consider the subject-matter how different it seems from that which we encounter in the other parts of the original process. So far is this the case that some people seem to consider that the scientific method cannot be pursued at all in this sphere, others appear to resent any such attempt, and yet others to believe that the attempt is doomed to failure. The 'chain of events' so far considered before we come to ask what happened in the man's mind are all events which, as we say, take place in space and time. In the investigation of motives we can still talk intelligibly of time—the motive may have originated at a period relatively remote, and the whole act may have been deliberately planned, or the man may have acted 'on the spur of the moment'. But what use can we make of the notion of space? Can a motive have a position 'in' space? Does it possess extensive magnitude? What relation does it bear to spatial things? Is it a thing in the same sense (whatever that may be), and if not what sort of an entity is it? These are some of the questions which arise at this point and we shall return to them—in so far as they concern biology—in Part II, and some of them will arise in Part I. Meanwhile we must return to the question of the continuation of the physical analysis where we left it at the limit of perception.

The fact that the analytical procedure of science can be continued beyond the limits of perceptual analysis leads us to consider a little what is involved in the postulation of hypothetical entities. The type of such things which at once comes to mind is the material particle or corpuscle of the physical sciences. This has been employed in two principal ways. On the one hand it has been used for the extension of mechanics into the infra-perceptual world, and on the other hand, for the explanation of the chemical changes of bodies. In both cases material bodies are regarded as *composite* as contrasted with their apparent homogeneity to our senses. In their employment in mechanics the particles have accounted for such phenomena as the pressure and expansion of gases, these being conceived as resulting from the motions of the particles which, for this

purpose, may be regarded as ultimate. But in their application to chemistry such particles are considered to be themselves particulate, and the various changes which bodies are observed to undergo, of the kind distinguished as chemical, are interpreted as resulting from the decomposition of such particles and their recomposition into other forms from the more ultimate ones.

Now in both these cases these particles can be represented in the imagination, i.e. we can form images of them. But we can only form images on the basis of perceptual experience. We can only form an image if at least the elements of which it is constituted have been given in perception. And such images are usually formed on the basis of visual perceptual experience. Incidentally since all things given in perception are spatial things, all images we care to make to ourselves will also be images of spatially extended things. Now it has been the custom to represent the particle of mechanics as a minute sphere, and the atoms of the chemist are similarly represented. But the image will have more than shape. It will, for example, have colour and perhaps other qualities which, however, are not essential to its use in scientific explanation. On the other hand, the physicist endows his particles with properties which cannot always be represented by the image. For the purposes of mechanics they need only possess mass and velocity. Similarly the chemist endows his particles with 'chemical affinity' or 'attraction'. In the early days of the atomic theory this was represented in the imagination by hooks and roughnesses, just as it is still represented by lines or 'bonds' in our formulæ. But such primitive realism has long been abandoned, and the place of hooks taken by electric charges which cannot be represented by an image. Even if we think further about the shape of these particles we see that the image which represents them as a sphere is inadequate. A spherical molecule may satisfy the requirements of mechanics, but will be repudiated by the chemist, who will point out that in most cases it will be composed of at least two atoms. Similarly modern theoretical physics requires that the atom itself should be particulate. Thus we see that an imaginary particle which is adequate for one science, or for one stage of the development of a science, may not be at all adequate for another.

Now we have to inquire into the rôle of these hypothetical

particles in explanation. How do they stand in regard to the three explanatory relations of resemblance, number and causation? In so far as we can make images of them they must resemble perceptual objects, and in this sense will carry 'intelligibility' into the infra-perceptual world. We can understand how a gas exerts its pressure on the walls of the containing vessel if we imagine it as composed of myriads of minute particles in motion constantly hitting the walls, because we know what it would be like to be pelted with tennis balls. But the corpuscular hypothesis enables us to do more than this. The particles can be treated like visible material bodies in accordance with the Newtonian laws of motion, and numerous properties of gases can be predicted and many empirical laws brought together, e.g. Boyle's law, the laws of diffusion, Avogadro's hypothesis, etc.—all as the result of the statistical effect of particles endowed with mass and velocity. Moreover we see that this result is reached independently of the images we may choose to frame of these particles. All that is required is that they should be numbered, and that their average mass and velocity should be in some way ascertainable.

We now come to causation. This seems to fall out of view in the calculating process: given particles in motion and no more seems to be required. But that something else is believed to be required we have already seen from the passage quoted above on p. 46, where we read that: 'Energy is the underlying cause of all changes in matter.' Thus the notion of energy appears to be introduced when we ask what is responsible for the movement or the chemical arrangement of the particles. Temperature changes accompany chemical transformations and we say that heat—a 'form of energy'—is absorbed or evolved. The particles in gases are said to 'possess' kinetic energy which is 'given up' to the wall of the vessel on impact and manifests itself as heat or mechanical work. Thus over and above the particles and their motions something else seems to be necessary. This entity is also said to be measurable and so finds a place in the calculating process. But energy cannot be represented in the imagination because it is not a perceptible entity. We see the billiard ball rolling and feel it strike the hand but these are supposed to be the effects of the energy, not the energy itself. Thus we are able to think about energy without being able to

imagine it. In other words we possess a concept of energy which can play its part in the explanatory process quite unrepresented by an image in the sense in which we have been using that term. Now the question arises: What exactly has been happening with those entities which could be represented by images? We saw that such images are liable to be misleading, and now it seems probable that they were only representative of concepts, and that perhaps it was these concepts which have been doing the real work of explanation and that the images merely lent an aid to intelligibility in virtue of their resemblances to perceptual things. This is actually the case: material particles and atoms are concepts, and the images we form of them are only misleading representatives of them in the imagination. They may even lead to an incorrect use of the concepts. Thus Prof. M. Schlick says that the use of images is no hindrance so long as their representative function is remembered and so long as we know *what* they represent. But this is not always easy and hence:

> "The representation of concepts through images has been the most fruitful source of errors in the thinking of all philosophers. Thought flies forwards without testing the carrying power of its wings, without seeing whether the images which carry it are fulfilling their proper function. That must be established by going back to the definitions. But not seldom usable definitions are lacking, and the philosopher continues the flight with images which are supported by no firm conceptual framework. Wandering and premature downfall are the results."[1]

But now the question arises: If we drop our images what becomes of 'intelligibility'? Clearly there is more in this question of explanation than 'meets the eye'. Can we expect no more of science than formulæ with which we can calculate the course of events, or some of them? We evidently require to learn more about the nature of concepts and their rôle in thinking. Meanwhile we may note at this stage what the consequences of these reflexions are for biology. We cannot expect these physical and chemical hypotheses to do more for us in biology than they do in the sciences in which they originated. In physics their great merit has been that they formed the starting point for the process of calculation, but so far in biology this is true only to a limited, although increasing extent. In chemistry they have aided the investi-

[1] *Allgemeine Erkenntnislehre*, 2te Auflage, Berlin, 1925, p. 20.

gation of chemical changes and consequently they have also been useful in the study of chemical changes in organisms. But there are corpuscular hypotheses in biology of quite another kind. These will be examined in detail in their proper place. It suffices to mention here that the same principles apply to them as apply to corpuscular theories in general. They involve concepts and any image we may think proper to make of such concepts is likely to be misleading.

It is interesting to note that modern physics has now reached a stage when its fundamental concepts are no longer picturable in the imagination. This is the case with the theory of relativity, and also with the latest developments of the doctrine of the atom. Thus Mr. Bertrand Russell writes :

> ' It is chiefly through ideas derived from sight that physicists have been led to the modern conception of the atom as a centre from which radiations travel. We do not know what happens in the centre. The idea that there is a little hard lump there, which is the electron or proton, is an illegitimate intrusion of common-sense notions derived from touch. For aught we know, the atom may consist entirely of the radiations which come out of it. It is useless to argue that radiations cannot come out of nothing. We know that they come, and they do not become any more really intelligible by being supposed to come out of a little lump."[1]

Thus when calculation reaches the highest degree of generality intelligibility in terms of images and hence familiarity disappears completely. Perhaps this is why exponents of other branches of science sometimes say such unkind things about mathematicians and physicists. They feel they have been ' let down '. Now we can appreciate the full significance of the words of Professor Eddington already quoted : ' Thus the ultimate concepts of physics are of a nature which must be left undefined, we may describe how they behave, but we cannot state what they *are* in any terms with which the mind is acquainted.' It would, I think, be more correct to say that such concepts can *only* be defined, and that such definitions are the real subjects of any propositions we may assert about them, any images that may be or may have been associated with them being irrelevant. In the development of physics there has been a succession of laws each representing the best generalization of the knowledge of the time. With each improvement in instruments of precision for observation has gone a revision and further generalization (in some cases) of

[1] *Outline of Philosophy*, London, 1927, p. 163.

the laws, each previous step being recognised as only an approximation. We impose a rigid scheme upon nature until our powers of observation reach a sufficient degree of refinement to show how far it is false. In a sense it is the errors of each scheme which have lead to the next step forward. If its rigid schemes were successful physical science would come to an end, and it would, perhaps, be described as having attained to what is sometimes called 'absolute truth'. Physics proceeds not so much by the discovery of truth, but by the elimination of untruth—so far, at least, as its major generalizations are concerned.

We see, then, that corpuscular hypotheses play a double rôle in the physical sciences. On the one hand, they have provided a starting point for the processes of calculation—letting in, so to speak, the thin end of the mathematical wedge. And on the other hand they have offered an explanation in another sense—rendering natural processes intelligible in the imagination. In the earlier days of physics these two rôles were not separated but advanced hand in hand. It was believed that not only were we by them enabled to calculate and so predict the course of nature, but that we were obtaining a 'deeper insight' into her doings. Since then, however, it has begun to be realized that the explanatory rôle of corpuscular hypotheses may lead us astray, but that whatever becomes of them the calculations that have been based upon them still remain, because the results of calculations can be verified, but the hypotheses regarding the nature of the corpuscles cannot. Hence there is a tendency in modern physics to concentrate more on processes and their calculation than on the nature of things—just as Galileo had done. Some people are thus inclined to say that all explanation is illusory and that all we can do is to calculate the course of nature, not to understand her. If this is true the consequences for biology are obvious. There has been very little calculation in our science, but a good deal of explanation. Much of it has been genuine biological explanation, i.e. using biological concepts, but some has involved the use of corpuscular hypotheses borrowed from the physical sciences. The fate of biological explanations of this type will therefore be wrapped up with the fate of the physical explanations on which they depend, consequently it is necessary for us to understand the present position in physical science that such explanations occupy. It will

also be clear from the above that there is much to be done by way of understanding the use of imagination and conception, particularly in regard to their use in the postulation of hypothetical entities. It is evident that physics and chemistry, having developed earlier than the other sciences, have, so to speak, set the fashion in this respect. The hypothetical entities of physical science have been imagined on the basis of perceptible physical objects. Consequently when biology began to develop it found an explanatory apparatus already to hand with which to pursue analysis. Had biology developed first it is at least possible that its infraperceptual entities might have been conceived on a biological model, and physics might then have employed the notion of the organized body for its analysis. Physics would then perhaps have borrowed from biology, instead of vice versa. It is interesting, therefore, to note that some biological concepts such as 'organization' and 'organism' are now beginning to intrude in physical science. But there are special reasons for this which will be examined later.

In developing the notion of particles with properties physicists have been guided by what philosophers call the category of substance, and from what has been said it will be understood why this category has been the subject of so much criticism from the standpoint of physical science in recent years. Similar criticisms have been directed against the category of causation. It will be part of the duty of Part I to discuss the nature of these extremely general notions, to study their use in the natural sciences, and to investigate the ways in which they have been operative in biology.

Before we leave, for the present, the question of the use of images in explanation, it should be pointed out that, in the last stage of the analysis of our original imaginary example of the man pulling the chair, when we come to consider the mental processes involved, images fail us altogether. We cannot form any image of the man's motives because they are never given to us in perception. We know them primarily by 'having' them ourselves, and we can form concepts and think about such things just as we do about the things of the perceptual and infra-perceptual worlds. But the fact that they cannot be spatialised is a stumbling block to many and accounts for the persistence of attempts to think of such things in spatial terms. We shall soon encounter some of the consequences of such attempts.

This introduction may now be brought to an end with a brief review of some of the principal points it has disclosed. I began by referring to those peculiarities of the biological sciences which depend upon the characteristic antitheses of biological thought. I then attempted to indicate the lines along which we should look if we are ever to overcome, or at least to understand, them. This has carried us over a wide field. This survey has already brought to light a sufficient number of instances of the shortcomings of specialism to show the necessity—urged at the beginning—on the part of those who interest themselves in the theoretical or interpretative aspects of biology, of not limiting their outlook to too narrow a field. We require to devote more attention to what I have called 'principles of systematization' and this means that we must invoke the aid of logic and epistemology. This is a lesson which might have been learnt from physics. Biologists have been eager enough to borrow concepts from physics but not patient enough to learn more of how they are used and what is their exact epistemological position. This divorce between the natural and the philosophical sciences—which as we have seen is traceable to the Cartesian dualism—has not been so acute in physics as in other sciences because physics has always been linked to the philosophical sciences by mathematics. And now that pure mathematics and formal logic are regarded as two aspects of one and the same discipline this union becomes firmer still. Biology has yet to discover the necessity for the virtue of patience. It is still in the metaphysical stage: too eager to press on to startling 'conclusions' rather than to devote critical attention to the purification of its concepts and making more sure of its foundations. The accumulation of data, still less the erection of speculative theories, is not enough. It is hoped that the truth and importance of this contention will become clearer in the following pages.

PART I

THE DATA OF NATURAL SCIENCE and PRINCIPLES OF SYSTEMATIZATION

CHAPTER 1

Phenomenalism and Kindred Doctrines

Phenomenalism has had a considerable influence on modern thought especially in scientific circles. This influence is to be seen not only in the writings of those who have studied phenomenalism and embraced its point of view, but it is also discernible in current writers who do not appear to be fully aware either of the source of their opinions or of the implications which the phenomenalistic doctrines commit them to. The leading spirit in this movement has been Ernst Mach. He comes nearest to being a 'pure' phenomenalist. But I shall also extend the term 'phenomenalist' to a number of other writers who profess beliefs which are similar, in their consequences for science, to this. The doctrine is best known in this country through Professor Karl Pearson. There are important differences between the views of Mach and Pearson but these do not affect their conclusions and their attitude towards science.

Mach's best known book *The Analysis of Sensations* was first published in 1885, but I shall use the fifth German edition as a basis for the following remarks. It should be said at once that it is extremely difficult to criticize Mach's views because it is impossible to pin him down to definite assertions. When we think we have found one and that we are beginning to understand the position we find in another place that what

seemed to be a definite assertion was, in reality, only intended to be expressive of a 'provisional point of view.' In fact Mach definitely says in more than one place that he does not offer a system of philosophy but only a provisional attitude in which science, freed from all awkward questions, will be able to pursue its investigations in peace. This seems to be a characteristic common to most of the phenomenalistic writers, and consequently the critic is committed to all the difficulties of a guerilla warfare. So true is this that Dr. Broad, in his examination of phenomenalism in *Perception Physics and Reality* is compelled, in order to criticize the doctrine, to *invent* arguments for it himself!

In the Preface to the fourth German edition of *Die Analyse* the Author states:

> 'When, about thirty-five years ago, I succeeded, by overcoming my own prejudices, in firmly establishing my present position and in setting myself free from the greatest intellectual discomfort of my life, I attained thereby to a certain satisfaction.'[1]

This ability to give freedom from intellectual discomfort, is, as we shall see, one of the great claims of phenomenalism. We shall find that the discomfort in question arises from an appreciation of the difficulties of certain problems around which metaphysical inquiry has turned since the beginning of the modern era. Those who have not experienced this discomfort will not, of course, feel much need for the solace offered by phenomenalism, and it will be difficult for them to take much interest in a criticism of that doctrine unless the nature of the original problem is brought home to them. The question which seems to have occasioned so much discomfort to Mach is the question of the relation between the 'physical' and the 'psychical'. He believed he had found a way out of this difficulty which showed the problem to be an illusory one, and one which could, therefore, be safely neglected by science. All such problems he considers to be either non-existent or illusory and insoluble. Now it is, I think, very important to note that the starting point for Mach's inquiries was not the problem of knowledge—not an attempt to answer what I have called 'Mill's question' in order to find a firm foundation for knowledge—but an attempt to overcome a *metaphysical* difficulty which had occasioned him 'intellectual discomfort'. But it was by means of a theory about knowledge that he

[1] Eng. trans., Chicago and London, 1914.

tried to do this, and that is why his views are important for the present inquiry. Nevertheless it is well to bear in mind the metaphysical motive which underlies it.

Mach begins by examining the notion of a physical 'object', 'body', or 'thing'. He employs much the same kind of arguments as have been familiar since the time of Berkeley to show that all claims to knowledge of a persisting thing apart from its colour, shape, and other qualities is illusory and idle. Therefore he considers that we should be content with regarding it as simply a collection of 'sensations' or 'elements', as he calls them, which happen to be momentarily connected together, without attempting to account for why they go together. That attempt he considers to be extra-scientific or metaphysical.

> 'We see an object having a point S. If we touch S, that is, bring it into connexion with our body, we receive a prick. We can see S, without feeling the prick. But as soon as we feel the prick we find S on the skin. The visible point, therefore, is a permanent nucleus, to which the prick is annexed, according to circumstances, as something accidental. From the frequency of analogous occurrences we ultimately accustom ourselves to regard all properties of bodies as "effects" proceeding from permanent nuclei and conveyed to the ego through the medium of the body; which effects we call sensations. By this operation, however, these nuclei are deprived of their entire sensory content, and converted into mere mental symbols. The assertion, then, is correct that the world consists only of our sensations. In which case we have knowledge only of our sensations, and the assumption of the nuclei referred to, or of a reciprocal action between them, from which sensations proceed, turns out to be quite idle and superfluous.'[1]

It must be understood that Mach does not deny the great practical simplicity and utility of the notion of the physical thing, but he considers that it is scientifically inadmissible because it leads to unsurmountable philosophical problems. He next directs the same sort of criticism against the notion of a persisting 'self', 'ego', or 'subject' who is ordinarily said to 'have' or 'sense' or 'experience' these sensations, as Mach calls them.[2] This, too, he considers, consists, on an ultimate analysis, of a series of 'elements' or sensations, composed, either of those alone which we call private or 'subjective' experiences, such as pleasure and pain, desires, etc., or of those in conjunction with the sensations of colour,

[1] *Op. cit.*, Fifth German edit. (1906), p. 10.
[2] He uses this term for what are now usually called *mens*.

touch, etc., which we suppose ourselves to perceive by t of the bodily senses. But in these cases, too, all we know are the 'elements' and there is no need to suppose any sort of enduring self or ego who 'has' them.

> 'The primary fact is not the ego, but the elements. . . . The elements constitute the I. If I have the sensation green, signifies that the element green occurs in a given complex of other elements (sensations, memories). When I cease to have the sensation green, when I die, then the elements no longer occur in the ordinary familiar association. That is all. Only an ideal mental-economical unity, not a real unity, has ceased to exist.'[1]
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> 'If a knowledge of the connexion of the elements (sensations) does not suffice us, and we ask Who possesses this connexion of sensations, Who experiences it? then we have succumbed to the old habit of subsuming every element (sensation) under some unanalysed complex, and we are falling back imperceptibly upon an older, lower, and more limited point of view.'[2]
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> '. . . if we take the ego simply as a practical unity, put together for purposes of provisional survey, or as a more strongly cohering group of elements, less strongly connected with other groups of this kind, questions like those above discussed will not arise, and research will have an unobstructed future.'[3]
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> * * * * * * * * * *
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> 'For us, therefore, the world does not consist of mysterious entities, which by their interaction with another, equally mysterious entity, the ego, produce sensations, which alone are accessible. For us, colours, sounds, spaces, times . . . are provisionally the ultimate elements, whose given connexion it is our business to investigate. It is precisely to this that the exploration of reality consists. In this investigation we must not allow ourselves to be impeded by such abridgments and delimitations as body, ego, matter, spirit, etc., which have been formed for special, practical purposes and with wholly provisional and limited ends in view.'[4]

These passages will suffice to convey the essentials of Mach's doctrine. The whole contents of human experience—whether it be the experience of the external world which we appear to share with others, or the experiences which are private to ourselves—are thus all analysed into elements which are all declared to be ultimately of the same nature. Consequently 'the great gulf between physical and psychological research persists only when we acquiesce in our habitual stereotyped conceptions.' All awkward questions thus disappear, and the task of science stands out clearly as one

[1] *Ibid.*, p. 19.
[2] *Ibid.*, p. 20.
[3] *Ibid.*, p. 23.
[4] *Ibid.*, p. 24.

simply of describing the connexions of these elements. If we happen to be investigating the connexions between elements both belonging to what in our bad old habits we call the external world then we are pursuing physics. But if one lies in the outside world and the other ' passes through my skin ' (p. 16) then we are psychologists. There is, according to Mach, no fundamental difference that need trouble us—it is all a matter of connexion between elements. The only point of importance which is to decide between one description and another is one of economy. The object of science is to effect an economy of thought. On p. 29 Mach says :

> ' The biological task of science is to provide the fully developed human individual with as perfect a means of orientating himself as possible. No other scientific ideal can be realized, and any other must be meaningless.'

It is extremely difficult to find anything positive in all these assertions. They consist for the most part of denials based on traditional arguments which Mach takes over from his predecessors without any attempt to discover a flaw in them. Mach does not assert consistently that only the ' elements ' *exist* but that these are all that we *know* plus their connexions. But these connexions are themselves, according to Mach, also ' sensations '—at least in the case of spaces and times, and hence only elements among other elements. How we classify these elements is, for Mach, purely a matter of convenience. The method of everyday life, which regards one set as qualities and properties of physical bodies, and the other as belonging in some way to a ' person ' or ' self ', is, according to Mach, quite indispensable—why he does not say—for our daily needs, but useless for science because it leads to insoluble difficulties. Science must therefore treat them all on the same footing, and find a way of dealing with them which is independent of these assumptions, and the only guiding principle we require for this purpose is that of ' economy '—a principle which plays a great part in phenomenalistic writings. If two schemes are equally successful in accounting for the connexions of the elements, and in predicting what sensations ' we ' are to ' expect ' (whatever that may mean), preference is to be given to the one which is most economical of thought. This throws light on that intellectual discomfort to which reference has already been made. It is because they cost him so much intellectual labour that Mach objects so strongly

to the doctrines he wishes to overthrow. Science is offered as an intellectual labour-saving device.

Some critics represent Mach as holding the view that only sensations exist: that reality simply consists of sensations (in his sense) and nothing more. If this were the case we should know exactly where we stood with Mach, and it would not be difficult to criticise him. But if this is the case it seems impossible to understand what he means when, for example, he says, as in the passage last quoted on p. 89, that the biological task of science is to provide 'as perfect a means of orientating himself' as possible for the 'human individual.' It is true that Mach does not say to *what* the human individual is to orientate himself, but as he speaks of the biological task of science he presumably means his environment. Now although he has said that the environment is to be regarded as a series of 'elements' and the human body and 'ego' as also a series of elements of the same fundamental nature, he has also said that this is only a 'provisional' point of view for the purpose of mental economy and for the avoidance of intellectual discomfort. Consequently, when he speaks of the human individual orientating himself to his environment he is presumably to be understood in some realistic sense, otherwise what *does* he mean? What can be meant by one set of sensations as such orientating itself to another set? Again Mach frequently appeals to the doctrine of evolution to account for various human mental characteristics. For example, he wishes to show that the pursuit of science is a development from processes which are to be recognized even in much less highly-evolved animals. But what meaning can we give to evolution if it is not to be understood realistically, i.e. as a process which has happened in real animals and plants at a time when there were no human individuals to know and think about them? If evolution is only an explanatory fiction for accounting for the connexions between sensations (which is the case with all scientific explanations according to Mach) then we cannot employ it as if it were *more* than a fiction for economizing thought. On the other hand, in interpreting it realistically we are clearly going beyond our immediately given 'sensations' because no one, not even a phenomenalist, has contended that evolution is a 'sensation.' It is an interpretation. But an interpretation of what? A phenomenalist must answer: of sensations, since these are

all we know. In that case it stands on exactly the same footing as other scientific interpretations, i.e. it is simply a device for describing the connexions of elements and beyond these elements there is nothing, if as Mach says: 'The assertion, then, is correct that the world consists only of our sensations.' I find it quite impossible to reconcile Mach's doctrines with his use of the notion of evolution. We shall encounter the same difficulty in other phenomenalistic authors. In regard to this question they seem to wish to have the best of both worlds. It is interesting to note what F. H. Bradley has said on this point.

> 'And, when we hear of a time before organisms existed, that, in the first place, should mean organisms of the kind that we know; and it should be said merely with regard to one part of the Universe. Or, at all events, it is not a statement of the actual history of the ultimate Reality, but is a convenient method of considering certain facts apart from others.'[1]

Thus, according to Bradley, evolution is to be interpreted phenomenalistically. He recommends phenomenalism as a doctrine *for science*. Science, apparently, is not able to tell us anything about reality. That is solely the business of metaphysics. The province of natural science 'does not fall outside phenomena.' And 'in order to understand the co-existence and sequence of phenomena, natural science makes intellectual construction of their conditions. Its matter, motion, and force are but working ideas, used to understand the occurrence of certain events. To find and systematize the ways in which spatial phenomena are connected and happen—this is all the mark which these conceptions aim at.' Thus 'while metaphysics and natural science keep each to its own business, a collision is impossible.' But Bradley, while thus recommending phenomenalism as the proper standpoint for natural science proceeds next to show the inability of such a doctrine to account for all the facts of experience, i.e. to undertake the task of philosophy. But Mach makes no such claims. He expressly says: 'Es gibt vor allem keine Machsche Philosophie.' He repeatedly states that it is only a provisional attitude for *science*, although he does not attempt to conceal his belief that no other attitude is possible. Moreover there are professed philosophers who have worked out a complete phenomenalistic system which has received Mach's

[1] *Appearance and Reality*, 2nd ed., London, 1908, p. 184.

warm approval. How then will they answer Bradley when he concludes with these words:

> 'But when Phenomenalism looking its head, and, becoming blatant, steps forward as a theory of first principles, then it is really not respectable.'[1]

If therefore we interpret Mach to be offering a provisional attitude for science without further claims why does he admire Avenarius and Schuppe so much who certainly claim their doctrine as a 'theory of first principles'? And if it is not such, how can Mach feel he has overcome that 'intellectual discomfort' which cost him so much mental effort, so contrary to his principle of economy? Here again we seem to meet the same desire to have the best of both worlds. And with regard to Bradley's attitude towards science the difficulty seems to be this: If scientific theories are *only* devices, 'intellectual constructions', to enable us to systematize 'the ways in which spatial phenomena are connected and happen' what use can metaphysics make of them? And if metaphysics is not to work with the data of the natural as well as the philosophical sciences, subject, of course, to logical and epistemological criticism, what is it to work with? Much depends, of course, on what is meant by 'spatial phenomena' and we shall come to this presently, but to take a particular case: The doctrine of evolution is either true or not true. It may not be easy to decide between these alternatives but *if* it is true then it *is* true whether you are talking biology or metaphysics.

Let us leave Mach for the present and turn to the views of Karl Pearson as set out in the *Grammar of Science*. He begins by analysing for us our perceptual knowledge of a blackboard. He points out the various visual and tactile elements which reflexion shows to enter into the object we call a blackboard in adult life. He calls them sense-impressions. He says:

> 'The sense-impressions which determine the reality of the external object may be very few indeed, the object may be largely constructed by inference and association, but some sense-impressions there must be if I am to term the object real, and not a product of my imagination.'[2]

[1] *Appearance and Reality*, 2nd ed., London, 1908, p. 126.
[2] *The Grammar of Science*, London, 1911, p. 49.

Pearson now undertakes to do what, according to Mach we must *not* do: he attempts to account for the *origin* of these sense-impressions. He says that as a result of some occurrence in the material world a 'message' is carried from the sense organ along a sensory nerve to the brain, and that 'at the brain what we term a sense-impression is formed' and, as a result of physical changes in the brain, may be 'stored.' Now in addition to all this Pearson says that there is also 'seated' in the brain an 'ego' which takes notice of these impressions, and he likens this ego to a clerk seated in a telephone exchange and listening to the messages coming over the wires. This is reminiscent of the teaching of Locke with his sheet of white paper upon which the senses write their 'ideas' as he called them, or his dark room which they from time to time light up. It should be sufficiently obvious to a modern reader that these are crude analogies borrowed from ordinary experiences of the physical world, and not to be taken too literally and applied without caution to a relation about which we know next to nothing. Such a procedure did well enough in the seventeenth century, but we might expect some improvement in our notions about these matters when we come to the twentieth. But Pearson seems to be quite satisfied with the analogy of the telephone clerk, and on these assumptions he is compelled to conclude that we can know very little indeed about the external world. We can get no nearer to it, he says, than the 'brain terminals of our sensory nerves'. We are even worse off than the telephone clerk because we have never been outside our office:

> 'Very much in the position of such a telephone clerk is the conscious ego of each one of us seated at the brain terminals of the sensory nerves. Not a step nearer than those terminals can the ego get to the "outer world", and what it and for themselves are the subscribers to its nerve exchange it has no means of ascertaining' (p. 61).

But this ego is not a mere passive spectator of these impressions. It 'projects' some of them outwards and calls them the external world, it also 'classifies these impressions, analyses or simplifies their characteristics, and forms general notions of properties and modes.'

Now all this has at least a superficial appearance of being firmly founded on 'objective' science and of being quite free from the taint of 'metaphysics' or any nonsense of that kind.

as indeed its author claims it to be. Upon what science, then, is it founded? First, it is clearly based on physics and physiology, but obviously not solely on physiology because physiologists contend that they have no concern with anything but the physical and chemical processes which go on in organisms, and clearly 'egos' and 'sense-impressions' do not come within the scope of either physics or chemistry. As we saw in the Introduction these sciences abstract from such constituents of our experience. Mach, as we have seen, repudiates the 'ego' entirely. Thus Pearson has not only invoked physiology but also psychology, and his theory of sense-impressions involves a passage from one to the other.

Now consider the consequences of this theory. You are, and can be, aware of *nothing* but certain events which are happening at the sensory terminals of your brain. But of course they do not *seem* to have anything to do with your brain. Neither do you seem to be in your brain or to know that you have got a brain. Although your 'conscious ego'—the 'part' of you which does the knowing—is inside your brain it cannot know your own brain, but only other people's brains, and from this it infers that it is inside a brain. But of course it does not really see other peoples' brains or even know that they have them. All it knows is its own sense-impressions. But it cleverly interprets some of these—by its powers of 'projection'—as meaning that there are other people with brains. It also interprets other impressions to mean that it has got a body. And from these data it infers that it is inside its own body. It is just as though the telephone clerk—born and bred in his office—should infer from the noises in his telephones (which is all he is aware of in the office) that offices existed and that he was in an office. It is thus extremely difficult to understand how, if we are shut up in the way Karl Pearson says we are, we should ever suppose that our sense-impressions have anything to do with an external world at all. Why should we project them? What does it mean if we do, if we can be aware only of sense-impressions? Is it not possible that people who think with Karl Pearson have *invented* 'projection' in order to overcome the dire consequences of their own 'introjection'? It must be remembered that, as Pearson himself insists, this doctrine applies to all sense-impressions, so that we cannot appeal to tactile impressions to help us to an external world. Nothing but the

miraculous power of projection will do this, and, on Karl Pearson's principles, it is difficult to see how it does it. But now it is instructive to hear what Mach says about 'introjection'. In a footnote on p. 22 of *The Analysis of Sensations* he quotes the following passage from Hering in Hermann's *Handbuch der Physiologie*:

> 'The material of which visual objects consist is the visual sensations. The setting sun, as a visual object is a flat, circular disk, which consists of yellowish-red colour, that is to say of a visual sensation. We may therefore describe it directly as a circular, yellowish-red sensation. This sensation we have in the very place where the sun appears to us.'

Mach then comments on the objections of the people 'who have not come to close quarters with these questions by serious thinking' and who therefore 'pronounce this way of looking at the matter to be mere hair-splitting.' He then explains:

> 'Of course, what is chiefly responsible for their indignation is the common confusion between sensible and conceptual space. But anyone who takes his stand as I do on the economic function of science, according to which nothing is important except what can be observed or is a datum for us, and everything hypothetical, metaphysical and superfluous, is to be eliminated, must reach the same conclusion.'

Mach then quotes with approval the following from Avenarius:

> 'The brain is not the dwelling-place, seat or producer of thought; it is not the instrument or organ, it is not the vehicle or substratum, etc. of thought.' . . . 'Thought is not an indweller or command giver, it is not a second half or aspect, etc., nor is it a product; it is not even a physiological function of the brain, nor is it a state of the brain at all.'

And Mach adds: 'The method which he (Avenarius) terms, "The exclusion of introjection", is only a particular form of the elimination of the metaphysical.' Now one striking feature of the two phenomenalistic writers we have been considering is that neither appears to have the slightest doubt that his opinions are *right*. They are given to the world as the only ones possible for any sane man who is intelligent enough to understand them and is not blinded by his own prejudices. And yet they flatly contradict one another. Therefore at least one author cannot be right in spite of the strength of his convictions. Each claims to be 'antimetaphysical' and yet Mach is condemning introjection as meta-

physical, and in every way opposing the assumptions which Pearson, who quotes Mach with approval, and borrows a figure from his book, takes to be solid non-metaphysical foundations. On p. 42 and the immediately following pages of Mach's book there are further references to introjection, too long to quote, in which its origin is explained and its evil consequences are exposed. I mention these opinions in order to show how fundamentally Mach's view differs from that of Pearson.

We can now return to the *Grammar of Science* and see what are the consequences of introjection there. Karl Pearson's chief difficulty is obviously to show how we come to suppose there is an external world at all. He began by assuming we had a good deal of knowledge about it but his conclusions compel him to believe that we can have none at all. He makes the following assertions about the external world: (1) that it exists; (2) that our sense-impressions 'symbolize' it, but that (3) they do not resemble it, and (4) that the things of the external world 'produce' sense-impressions, but we must not say they 'cause' them. On this point Pearson is by no means clear. In order to 'introject' in the first instance—as in his story of the blackboard—he assumes that events outside the body excite the sense organs, but later on he writes:

> 'Of what lies beyond them,[1] of "things in themselves" as the metaphysicians term them, we can know but one characteristic, and this we can only describe as a capacity for producing sense-impressions, for sending messages along the sensory nerves to the brain. This is the sole scientific statement which can be made with regard to what lies beyond sense-impressions.'[2]

But now Karl Pearson proceeds to contradict this:

> 'But even in this statement we must be careful to analyse our meaning. The methods of classification and inference, which hold for sense-impressions and for the conceptions based upon them, cannot be projected outside our minds, away from the sphere in which we know them to hold, into a sphere which we have recognized as unknown and unknowable. The laws, if we can speak of laws, of this sphere must be as unknown as its contents, and therefore to talk of its contents as *producing* sense-impressions is an unwarranted inference, for we are asserting cause and effect—a law of phenomena or sense-impressions—to hold in a region beyond our experience. We know ourselves, and we know around us an impenetrable wall of sense-impressions. There is no necessity, nay, there is want of logic, in the statement that behind sense-impressions there are "things-in-themselves" producing [illegible]'

[1] I.e. beyond sense-impressions.
[2] *Grammar of Science*, 1911, p. 67.

Now how is it possible to reconcile all this with the original account of the origin of our sense-impressions in the case of the blackboard? Clearly we have no ground on Karl Pearson's principles for asserting anything about anything beyond our sense-impressions, not even that it exists, consequently we can say nothing about brains and sense organs nor about blackboards and physical stimuli. And consequently we have destroyed the ladder by means of which we climbed up to this position. If you wish to be really consistent in limiting human knowledge to sense-impressions you have no good grounds for believing in *anything* but those impressions, in other words you have no alternative but to be a solipsist. Now both Mach and Pearson agree in confining human knowledge to sense-impressions, and also in wanting to avoid solipsism, but they differ totally in their methods of escape from it. Pearson tries the Kantian method of a 'thing-in-itself' which is unknowable except in so far as it is required to act as a go-between to account for the fact that two people can talk about the same thing, but he gets into great difficulties in trying to adhere to this view because he thoroughly dislikes the 'thing-in-itself' since it obviously contradicts what he believes about sense-impressions. Mach more consistently will have nothing to do with it. He boldly asserts that we know nothing but sensations, and then calmly allows us to infer that there are other 'egos' from the behaviour of their bodies![1] Mach, of course, professes not to introject and so he does not have to project. For him sensations are not experienced by conscious egos, they simply float about loose in transient connexions with other sensations. Compare, for example, what he says about the meaning of 'I have the sensation of green' in the passage quoted on p. 88. But we saw how difficult it was to take this seriously because it then becomes difficult to see why loose associations of sensations should trouble about being 'economical' of thought, or indeed what place there can be for thought or science at all. But in that case what becomes of Mach's escape from intellectual discomfort? He has offered a provisional attitude for science —a 'working' hypothesis. But this does not stifle thought: it leaves the old difficulties just where they stood. What then has Mach accomplished?

[1] Mach has an argument on this point in *Erkenntnis und Irrtum*, 3te Auflage, Leipzig, 1917, p. 9.

The next phenomenalistic writer to be discussed is the celebrated physiologist, Max Verworn, who expounds these doctrines in his *General Physiology*.[1] He begins with a criticism of du Bois Reymond's views on the limits of our knowledge of nature. Du Bois Reymond held that the aim of natural science was the 'reduction of changes in matter' or the 'resolution of natural events' to the mechanics of atoms. The first limit to our knowledge was, he believed, set by the atom itself, because what it was we could not learn since it only has the properties which we ascribe to it on the basis of the perception of what large bodies show us. Du Bois Reymond's second limit was constituted by the impossibility of passing from matter to mind. On these questions, he believed, we must content ourselves with *Ignorabimus*. Verworn says that the first requisite of knowledge is the assumption that something exists. He asks: What is real? He raises doubts about the view that 'The physical world existing outside of us and independent of our own minds is real, and that, accordingly we must reduce all phenomena to its laws.' He concludes that without doubt

> '. . . we actually see the body, others see it; and we say it exists. We are right; without doubt it exists, but it does not exist outside our mind . . . the physiology of the senses shows that all that comes in through the door of the senses affords us simply and solely, sensations.'[2]

Beyond these sensations, he says, we can know nothing concerning an external world, and can only form hypotheses. Verworn thus gets his 'sensations' into his head by much of the same sort of arguments from the physiology of the sense organs as were used by Karl Pearson. He now asks:

> 'What is the thing outside my mind that produces in me through the senses this idea? In other words, what is the external world?'[3]

This question is answered in the following remarkable argument on the same page:

> 'As is well known, natural science has shown that every phenomenon in the physical world has as its cause another physical phenomenon. . . . Hence the cause of my sensation of the physical

[1] See the Eng. translation from 2nd German edit., 1899.
[2] *Op. cit.*, p. 36.
[3] *Ibid.*, p. 37.

> to another sensation, which is located not outside of but within my mind . . . causality itself, like all other sensations, ideas, conceptions, or whatever we may term it, exists only in our own mind. If, therefore, the cause of my idea of the physical is located within, the supposition of a reality without is wholly unjustified.'

Verworn thus makes the same mistake as Karl Pearson of coming to conclusions which flatly contradict the premises by which he reached them. He begins with the physiology of the sense organs regarded quite realistically, as physiologists always do regard them when they are not philosophizing—calling them 'doors' through which something 'comes in' and 'affords us' sensations. He ends by saying that one sensation 'in the mind' *causes* another, and that belief in external reality is unjustified. How is it possible to harmonize these assertions? What does he mean by 'outside' and 'inside'? When he speaks of something coming in through the senses he presumably means *spatially* into the body. When he says in the mind what does he mean by 'in'? This is not explained. On the same page Verworn complains that various philosophers have 'endeavoured to base the reality of an external world upon the causality of phenomena', and says that 'the argument presents the rare spectacle of an attempted proof of something by means of that which is to be proved' but he apparently fails to see that his own argument presents the astonishing spectacle of trying to prove a belief to be false by premises which assume it to be true.

Verworn so far misunderstands Descartes as to suppose that he can be invoked in support of his arguments. He says (p. 36):

> 'More than two hundred years ago Descartes made the fundamental fact, that the whole physical world is only an idea, the starting point of his philosophy.'

The truth is of course that Descartes started the notion that knowledge of self is more certain than knowledge of the external world, but he certainly never denied either the existence of the latter or that we could have a great deal of reliable knowledge about it. Verworn also invokes Mach in spite of the fact that he has done the very thing which Mach protests against, i.e. 'introjection'.

> 'It is to be hoped,' says Verworn, on p. 38, 'that this monistic conception will gain ground more and more in science; it alone holds strictly to experience, it is not hypothetical, and it necessarily sets aside the ancient doctrine of the dualism of the body and the mind.'

It is strange that Descartes, who advocated the latter doctrine, should be invoked in support of the monistic one. Further cogitation leads Verworn to the remarkable conclusion that when we suppose ourselves to be pursuing natural science we are really practising introspective psychology:

> 'the laws of the physical world are the laws according to which our own psychological phenomena occur, because the physical world is only our idea. All science, therefore, is in this sense psychology'.'

He ought, of course, to say *my* psychological phenomena, because he has already (p. 37) rebuked those who appeal to other minds by saying that these are for him only bodies and therefore only ideas 'in his mind'. He ought of course to be a solipsist, but like the other phenomenalists he does not see this. Similarly, like the other phenomenalists he inconsistently appeals to evolution. He says for example:

> 'Our craving for causality arose and became established in the course of evolution by the continual reduction of effects to causes.'

But if evolution, like causality itself, is only an idea in Verworn's mind—and he has said that this is true of all 'ideas and conceptions'—then it is extremely difficult to see how my craving or your craving for anything could have arisen in the course of it. I once put this difficulty before a physiologist who was an admirer of Mach and Verworn, but 'he went away sorrowful' for he was of great (intellectual) possessions. He did not want to desert Mach and follow Darwin, who was as innocent of phenomenalism or any 'philosophy' as a new-born babe, neither did he want to desert Darwin for Mach. Can we have both?

The upshot of all this is that Verworn rejects du Bois Reymond's limits of natural knowledge: the first, because atoms are 'complex ideas' and therefore not atomic, and the second, because there is no difference between body and mind since body has become an idea in his mind.

One thing we can learn from phenomenalism, and that is to what an enormous extent learned men can thrive upon contradictions. Formerly the 'law of contradiction' used to be called a 'law of thought'. Modern logicians, or many of them, no longer take this view. It certainly is not a law exemplified by the thought of the phenomenalistic writers. It seems also to be a general characteristic of phenomenalists

that when once they have enunciated their doctrines they proceed at once to forget them, and then write just like ordinary mortals: thus giving the whole business an air of being 'moonshine'. Verworn manifests this characteristic with special clearness. On p. 46, for example, he contends that it is inadmissible to seek any but physico-chemical explanations for vital phenomena. He says:

> 'an explanatory principle can never
> with reference to the physical phenomena ....
> applicable in chemistry and physics to biological n

Now this, if it is intended to be laid down *a priori*, as it seems to be from the wording of this paragraph, is a sheer dogma. No one can tell what 'holds good' in physiology until he tries. To do otherwise would be to desert the empirical attitude of natural science. If it is held as a hypothesis well and good. But apart from this, when Verworn has said quite clearly and at some length that the only laws are laws of 'our own psychological phenomena' it seems idle to talk about any differences there may be between physical and physiological laws. It becomes quite pointless to discuss the 'vitalism versus mechanism' controversy when all laws have been melted down to laws of 'sensations'.

Verworn deplores the divorce between natural science and philosophy. He says:

> 'A severe blow will be inflicted upon the coming centuries, if the gulf between philosophy and science widens constantly from both sides; if, upon the one side, confused speculation, and, upon the other, narrow specialization constantly prevail, and prevent a mutual approach toward a beneficent common labouring-ground'[1]

With this I should heartily agree. But if Verworn's speculations, which have all the appearance of being 'confused', are a fair sample of philosophy, it is hardly surprising that natural science should feel little kinship with it. It is not realized that in order to diminish the gulf between science and philosophy it may be necessary to cultivate other 'habits of thought' than those which are found suitable in science. And, if this should prove too difficult, it is at least advisable to consult more than one authority and not pick out those which conform to one's opinions. Moreover, although in a sense science and philosophy have a common-labouring ground in

[1] *Ibid.*, p. 59.

so far as all men inhabit the same world, it would be a great mistake to suppose that their tasks were the same. But this has been discussed at sufficient length in the Introduction, and I suggest that some of the difficulties which we encounter in the phenomenalistic doctrines may be but a further illustration of the consequences of the limitation of the scientific outlook which were there pointed out. This will be further considered when we come to a general summing up and discussion of phenomenalism. Meanwhile there are other phenomenalistic writers to be examined.

The Italian mathematical physicist, F. Enriques, is a follower of Mach, only differing from him on minor points. His remarks on biology are interesting and well worthy of the consideration of biologists.[2] It is always helpful to see how our problems appear from the standpoint of another science, particularly as such opinions are usually disinterested. But Enriques' views on biological problems are not our immediate concern, and his epistemological opinions do not differ sufficiently from those of Mach to need special discussion. Among recent English writers Professor F. H. A. Marshall,[3] and Professor E. W. Hobson[4]—the first a physiologist and the second a mathematician—have declared themselves in favour of phenomenalism, but a phenomenalism very different from that of either Mach or Pearson. These two authors agree in the following opinions: (1) they both exclude psychology from natural science; (2) they both make a sharp distinction between natural science and philosophy; (3) they are both free from anti-metaphysical prejudices. Their attitude is therefore much the same as that of F. H. Bradley, and Professor Marshall quotes Bradley's view with approval. Professor Hobson considers that natural science should abstain from making any ontological assertions. That is to say it must not say that only sense-impressions exist, neither should it say that the various objects by means of which it explains phenomena exist. Such things, for example, as molecules and electrons,

[2] *Problems of Science*, trans. K. Royce, 1914, p. 367 *et seq.*
[3] In the paper already referred to above on p. 39.
[4] *The Domain of Natural Science*, Cambridge, 1923 (Gifford Lectures, 1921-2).

must not be regarded as really constituents of nature, but solely as products of thought. They may of course exist as entities discoverable in nature, but if so it is to be the business not of science but of philosophy to say so. Professor Hobson thus agrees with Mach and Pearson in holding that the task of science is simply to devise conceptual schemes for subsuming what is given to us in perception, without pretending that it is anything more than a device for economizing thought—conceptual shorthand, to use Karl Pearson's favourite expression. Professor Hobson calls his standpoint that of 'methodological phenomenalism'. In this form phenomenalism appears as a timid doctrine—letting "I dare not" wait upon, "I would", like the poor cat i' the adage', and either handing over the task of daring to philosophy, or declaring it to be impossible, according to the predilection of the individual author. The opposite extreme is illustrated by the following passage, quoted by Professor Hobson, from an article by Sir Oliver Lodge in *Nature*, Vol. CVI, pp. 796 *et seq.*:

> 'In such a system there is no need for Reality: only phenomena can be observed or verified: absolute fact is inaccessible. We have no criterion for truth; all appearances are equally valid; physical explanations are neither forthcoming nor required; there need be no electrical or any other theory of the constitution of matter. Matter is, indeed, a mentally constructed illusion generated by local peculiarities of space . . .
>
> 'But notwithstanding any temptation to idolatry, a physicist is bound in the long run to return to his right mind; he must cease to be influenced by superficial appearances, impractical measurements, geometrical devices, and weirdly ingenuous modes of expression: and must remember that his real aim and object is absolute truth, however difficult of attainment that may be, that his function is to discover rather than to create, and that beneath and above and around all Appearances there exists a universe of full-bodied, concrete, absolute Reality.'

Now as Poincaré has said it is equally easy to believe everything or nothing, the difficult thing is to think. And this is the only alternative that remains to us if we wish to avoid either of these two extremes. Phenomenalism is simply a system of negations. Dr. Broad says that Mach 'has no positive arguments for his position that are worth discussing'. But we cannot live on negations, any more than, as Professor Whitehead says, we can live on disinfectants. The 'principle of economy' which plays the central rôle in phenomenalism, if it has any meaning at all (and it is difficult to see what it means on the phenomenalistic view) seems to imply the belief

that all intellectual labour is essentially vain and idle. It is a counsel of despair and disillusionment. As biologists, if we accept phenomenalism as a basis for our investigations, we shall have to renounce all realistic interpretations for our explanatory schemes. The genes of the geneticist, and the doctrine of evolution itself, will have to follow the molecules and electrons of the physicist into the melting-pot of explanatory fictions. This is, of course, no argument against phenomenalism, nor in favour of a realistic interpretation of those scientific theories, but it does mean that we cannot *employ* such theories realistically in the way that phenomenalists themselves do. We simply cannot have our cake realistically if we have first eaten it phenomenalistically.

It seems then, that (assuming of course that we are not to give up thinking altogether) we have two principal alternatives: We can either embrace phenomenalism with or without a metaphysical backing which will be some sort of philosophical idealism; or, we must begin again and try to establish a tenable epistemological doctrine which will find a place for both the natural and the philosophical sciences in the same world, but without making either of them ridiculous. Anyone who is familiar with the history of modern philosophy will have no difficulty in understanding how phenomenalism came into existence. And at the time when Mach and Pearson first wrote their books the difficulties with which they struggled appeared in a very different light from what they do now. But this is no reason why natural science should continue to base itself on doctrines beyond which modern thought has already passed.

Before concluding this section a word should be said in reference to von Uexküll whose book *Theoretical Biology*[1] must also be called phenomenalistic. On p. xv the author writes:

> '*All reality is subjective appearance.* This must constitute the great, fundamental admission even of biology. It is utterly vain to go seeking through the world for causes that are independent of the subject; we always count up against objects, which owe their construction to the subject.'

This book is largely based on Kant's Transcendental Aesthetic, which appears to be the part of Kant's theory of knowledge which has least survived criticism. This does not, of course,

[1] English trans., London, 1926.

in the least imply that von Uexküll's book is without value but merely that its epistemological foundation should be born in mind in interpreting it. The subjectivism of the author is sufficiently plain in the above passage and also in the following which immediately precedes it on the same page :

> ' No attempt to discover the reality behind the world of appearance, i.e. by neglecting the subject, has ever come to anything, because the subject plays the decisive rôle in constructing the world of appearance, and on the far side of that world there is no world at all.'

In the next section I shall try to examine the theory of knowledge of Professor Lloyd Morgan as expounded by him in *Emergent Evolution*. Although Professor Morgan cannot be called a phenomenalist metaphysically speaking, his theory of knowledge has many features in common with those we have been discussing, and it does not appear to me to be tenable in conjunction with his metaphysics. Either one or the other should, it seems to me, be abandoned. This I shall try to show.

There is one important respect in which Professor Lloyd Morgan differs from the first three phenomenalistic writers we have discussed. He is not simply content with dogmatizing. He offers his view as a possible alternative to be examined on its merits—not as the only possible solution which everyone *must* accept or be branded with the word ' prejudice '. Neither does he claim—as Verworn does—that his view is ' not hypothetical '. Nevertheless it does seem to me that Professor Lloyd Morgan makes a fundamental error in method which is also found in the other phenomenalistic writers, and this is the mistake I shall try to make plain without attempting to discuss in detail the further working out of Professor Morgan's theory. As far as I can see he does not overcome what appears to me to be his initial mistake. But in this I may be wrong and I can therefore only recommend the reader to consult the original in Professor Morgan's book.

Professor Morgan's chief aim is to develop an evolutionary metaphysics. The scheme is outlined in the first chapter. Not until the second chapter does he say :

> this problem of knowledge—the relation of knowing to is thereby known."[1]

This means that unless we can satisfy the disinterested critic that our knowledge in general has certain characteristics, then that particular part of it which sets forth the doctrine of emergent evolution has no claim on our serious consideration. Thus Professor Morgan fully realizes the importance and urgency of this question, and although he does not actually begin his book with it, a great deal of the book is devoted to it. What, then, are the characteristics which our knowledge must have in order to do this? The doctrine of evolution in any form is a theory about the past history of certain things now in existence. It presupposes a knowledge of those things which is of such a nature that it permits us to make valid inferences about them and about their past history.

Everyone will, I suppose, agree to the following assertion of Alexander Bain:

> "It is a grave logical misdemeanour ever to give an inferior generality precedence of a superior; or to treat the two as of equal consequence, or even for a moment to be unaware of their relative standing."[2]

In relation to our present problem I interpret this to mean that if, for example, there is any doubt about the theory of evolution in general there cannot be *less* doubt about say, the evolution of the horse; and if there is any doubt about our knowledge of the external world then there will not be *less* doubt about our knowledge of part of it, e.g. the theory of evolution. This seems to me to be perfectly clear, and yet it appears to be just this principle that Professor Lloyd Morgan has forgotten. He does not seem to be in *any doubt about evolution*, but he is, apparently, in some *considerable doubt* about the existence of an independently real physical world or about what our sense-experience tells us about it (assuming that it exists). This is clear from the fact that Professor Lloyd Morgan uses arguments *based upon evolution* to throw doubts upon the reliability of our sense-experience.

[1] *Emergent Evolution*, London, 1923, p. 36.
[2] But is this the "vexed problem of knowledge?" See below, pp. 120, 130, 169.
[3] Bain, *Logic*, London, 1912, 3rd edit., Part II, p. 410.

At the same time he does *believe* in an external world, and unlike the phenomenalists, he allows us to know a good deal about it. But he does not think that this belief in an external world is justified by our sense-experience so he gets it in another way. Anything he requires for his metaphysics which is not justified by sense-experience he gets by what he calls 'acknowledgment'. In this way he gets (1) an external physical world; (2) God; (3) universal psycho-physical correlation; (4) other minds, and possibly other things, but we are chiefly concerned with the first. He says, on p. 33, referring to the acknowledgment of an external world:

> 'Why do I accept this *under acknowledgment*? Because I am not satisfied that its existence can irrefragably be established subject to the search-light of modern philosophical criticism. I admit then that in accepting it I go beyond the positive evidence. But I claim that it embodies nothing that is discrepant with, or contradictory to, that evidence.'

From this it seems perfectly clear that Professor Lloyd Morgan is or believes himself to be in doubt about the existence of an external world and consequently he cannot be *less* doubtful about any particular proposition regarding that world, e.g. that it or a part of it has evolved. It does not seem to me therefore to be a valid procedure to use arguments based on evolution in the way Professor Morgan does. We shall turn now and examine some of the things he says about knowledge. The following passage is strongly reminiscent of Karl Pearson's telephone clerk:

> 'On my view the mind is captain in the conning-tower of the bodily ship. It knows only such messages as come in from the world of battle around the ship. And the mind never gets outside its conning-tower of vision save through projicience.'

He explains that 'projicience' is to imply what the word 'projection' implied but more also. Not only 'out-there-ness' but 'all the objective characters with which a thing is clothed' are 'projicient in so far as they are the outcome . . . of the stimulation of the distance-receptors of the retina.' Thus Professor Lloyd Morgan, in spite of his doubts about the reliability of our sense perception, and in spite of his doubts about the existence of an external world, nevertheless assumes that we know quite a lot about physiology—just as the phenomenalists do. The problem is therefore, how on his

[1] *Ibid.*, pp. 30-1.

theory this is possible. On p. 50 he says that such qualities as hardness, coldness, slipperiness, taste, colour, and beauty are (1) 'projicient properties *referred* to the several objects of vision when we see them', and (2) 'conscious correlates of what occurs, on the plane of life, *within the person*—within that entity which is both body and mind. But as minded, they are projiciently referred to the objects of which we are conscious'. Now it is important to understand that by 'object' is not meant a constituent of the external world, but a mental 'construct'. On p. 48 Professor Morgan says that the mind 'is a participator, in accordance with its evolutionary status, in making the objective world what it is. Here I am at one with the idealists, though my line of approach is different from theirs.' Thus the objects to which this projicience is directed are both mental and inside the skull of the percipient. Whereas the older 'projectinnists' seem to have meant projection into an external world. I do not see at all how either variety will help to improve our relations with the external world. But here I may have failed to understand Professor Morgan. My difficulty will be further illustrated by the following passage on p. 51:

> "I shall call the place at which, as I acknowledge, a physical thing really is (its position in the non-mental world of such things) the *assigned place*; and I shall speak of the place to which the object given in perception is projiciently referred, as the *place of location*."

It is added that the two places normally 'coincide' but that this is not always so. But I do not understand how it is possible to *know* this if one 'place' belongs to the mental realm and the other to the physical, which is merely assumed or 'acknowledged'. In another part of the book Professor Morgan speaks of 'projicient reference' not to objects in his sense but to physical things or 'centres'. This is offered as an escape from solipsism which, he admits, would otherwise befall him. A second escape from this is also (p. 196) afforded by 'advenience' or 'advenient influence' by which appears to be meant the physical processes which occur in the external world and the percipient's body. I can perfectly well understand how all this would have to be taken into account by a theory of perception *if* you had all the physical and physiological knowledge that it implies. But if all that you can perceive is a series of constructs concocted from the mental

correlates of supposed cerebral processes—the very existence of an external physical world meanwhile being in doubt—I utterly fail to see how you could possibly obtain such knowledge. It seems plain that Professor Morgan does not *really* have any doubts about the external world at all nor about the validity of the theory of evolution and a great deal of physical and physiological knowledge. But if that is so why be so modest? Professor Morgan not only acknowledges an external world but also, as already said, he acknowledges God—thus suggesting that both are on the same epistemological footing. But there is this difference: Whereas *everyone* (except solipsists if there are such people) acknowledges an external world even if it consists of nothing but sensa (sense-impressions), only a relatively *few* people acknowledge God. Some people would like to acknowledge Him if they could, whilst others would not acknowledge Him at any price. What the correct interpretation of these facts is I do not know, but there is one point of interest in the present connexion. Most people would not feel the need of any assurance about the existence of an external world and they would regard it as quite superfluous to speak of assuming or acknowledging it. In the same way many people feel the same sort of assurance about God and arguments for and against His existence leave them just as cold as arguments about the existence or non-existence of the external world. It is just *possible* that *everybody* does in fact have some sort of *direct* cognitive relation to the physical world, but that only a few people have a direct cognitive relation with God. This would account for why it is so difficult for *everyone* not to believe in an external world, and how it is so difficult for a *few* people not to believe in God. But it is the theory of *some sort* of direct cognition of an external world which Professor Morgan is unable to accept, and his reason for not believing it is that he believes that cognition is an *evolved* function, and he thus commits the inconsistency which I pointed out at the beginning—the inconsistency, namely, of regarding the existence of an external world as doubtful and at the same time using arguments based on the occurrence of evolution in that external world. On p. 51 he writes:

> 'For long I accepted the widely current view that direct apprehension is an inalienable prerogative of the mind—something to be postulated *ab initio*. By slow degrees I came to realise that the genesis of apprehension is a problem—and a very difficult problem—which has to be solved.'

Now apart from the initial inconsistency already mentioned this seems a strange argument from an advocate of emergent evolution. The genesis of apprehension is certainly a problem but how can it be a problem antecedent to the problem of the possibility of knowledge of an external world? Moreover it seems to be an insoluble problem and one about which we can only speculate. How can we tell what it 'feels like' to be an amoeba, or what it 'looks like' to see with the eyes of a cuttlefish? But emergent evolution, if I understand it rightly, asserts that in the course of evolution a number of miracles happened—if by a miracle is meant something that is not understood. Two are of special importance: the emergence of 'life' and later of 'mind' (in one of the three senses in which Professor Morgan uses that term (p. 37)). These are spoken of as two 'qualities' expressive of new modes of relatedness at different levels, and it is explicitly stated (p. 204) 'that life cannot be interpreted in terms of physico-chemical relatedness only; that human affairs, which depend on the quality of mind, require something more than biological interpretation'. If now this is the case it is difficult to see why Professor Lloyd Morgan should stumble at 'acknowledging' what he calls 'direct apprehension' by mind on the human level. If his 'natural piety' leads him to accept two miracles why should he find the fact that the cognitive function has *evolved* a *reason for* being unable to believe it capable of doing what it *seems* to do—namely, to give the mind which exercises it a cognitive grasp of that which is other than itself? This is, of course, no argument in favour of 'direct apprehension' but it suggests that this should, on Professor Morgan's principles, present no special obstacles to him. He would not then have to 'acknowledge' an external world, and at the same time he would be saved from the fundamental 'logical misdemeanour' which was pointed out at the beginning. In other words if there is to be any acknowledging at all it would be simpler to acknowledge *some sort* of direct apprehension.

The difficulties and inconsistencies of the forms of phenomenalism so far discussed are perfectly familiar to philo-

sophers but, judging from the way in which they continue to be perpetrated by men of science, they do not seem to be so widely understood in scientific circles. It is for this reason that it has seemed desirable to devote so much attention to them, and for the same reason a few pages will be devoted to giving some elementary account of the philosophical ancestry of phenomenalism. It will then be easier to understand why it seemed a perfectly reasonable doctrine to its originators, and also why it need no longer seem so at the present day. The whole tendency to subjectivism which has dominated the last three centuries has rested on the belief, for which Descartes was responsible, that knowledge of self is prior to and more certain than any other knowledge. The form which this tendency took in the work of Berkeley, Hume and Kant came finally to dominate thought in the same way as the mechanics of Newton dominated scientific investigation. And just as further scientific investigation has led to a restriction of the scope of the Newtonian mechanics, so has further philosophical investigation shown that the view of knowledge which has so long been dominant is not so inevitable as it was formerly supposed to be. And as a further example of the same type of change it is perhaps worth noting, in passing, that it was believed that Euclid's geometry was the only possible geometry until Lobatschewski showed by constructing others that this restriction was erroneous.

At the time when phenomenalism arose it was naturally very difficult for thinkers to break away from the mentalistic or subjectivistic tradition, but that is no reason why people should go on taking it for granted at the present day. The psychological assumptions which were responsible for the developments which led ultimately to phenomenalism were, naturally enough, at the very birth of the sciences which deal with such matters, of a very crude description, and obviously influenced by simple physical ways of thinking. Sense-experience was conceived on the lines of a kind of magic lantern show. On the one hand were the physical world, the sense organs, and the brain—corresponding to the lantern and its parts. On the other hand was the mind corresponding to the observing audience. Between the two, like the screen with its pictures, stood the ' sense-impressions '—imprinted on the mind like images on a ' sheet of white paper.' This is the point of approach which underlies the doctrine of representative

perception.[1] Thus from the beginning what was immediately given in sense-experience was regarded as a sort of *tertium quid* between the individual mind and the common physical world. What was immediately apprehended in this way Locke called ideas. He then proceeded to apply this term to all the contents of knowledge, what was complex being regarded as compounded of simple ideas. Moreover the same term covered the experiences relating to sense and to the private world of thoughts, desires, 'feelings', etc.—the latter group being distinguished as 'ideas of reflexion.' From this it was but a short step to regard all 'ideas' as being 'in the mind' and to interpret this to mean that they were *mental*. This interpretation was given to the teaching of Locke by Bishop Berkeley who had no difficulty in showing that on such suppositions an outer world of *matter* to cause us to have these 'ideas' was superfluous. For if Locke's assumptions were correct we cannot know the lantern but only the pictures on the screen. Consequently we cannot compare the pictures with their supposed cause, and the hypothesis that the latter is 'inert matter' cannot be tested in experience and is therefore idle. Thus there were two questions. The first—Mill's question—what are the objects of intuition or consciousness? And the second was: What is nature? Locke and Berkeley both started with the first question. Locke tried to answer it by the help (in part) of certain assumptions concerning the second. Berkeley, adopting Locke's answer to the first question, at the same time giving it a certain twist of his own, came to conclusions about the second, namely about nature, which were not at all in harmony with what Locke had supposed. Thus Locke employed theories about the physical world in order to 'introject' and Berkeley profited by Locke's introjection although he himself really began, not with sense organs, but with 'ideas' which he *assumed* to be mental and to be dependent upon their being perceived. This very briefly and crudely was the path which led to Berkeley's idealism. It was idealistic for the reason just mentioned that it regarded what was *perceived* as mental, as well as the process of perceiving. But Berkeley argued further that as ideas alone were known, and as they were only

[1] For a good general account and criticism of representative theories see N. Kemp Smith, *Prolegomena to an Idealist Theory of Knowledge*, [illegible] 1924.

known to exist when they were perceived by some mind, then only minds and their ideas need or could be supposed to exist. But Berkeley's idealism was not *subjective* idealism. He did not believe that each person was shut up with his own private ideas, but that all ideas, when they were not perceived by us were perceived by some universal mind which he identified with the mind of God. It was God's will which determined the nature and order of our ideas, and what we call the laws of nature are the regularities in the order of these ideas.

Hume accepted the same sort of psychological foundations as his predecessors had laid. He made a distinction between what he called 'impressions', by which he meant the immediate experience of either sense or 'reflexion'; and 'ideas' which term he restricted to revivals of the impressions in memory. He seems to have believed in the realistic attitude of common sense rather than to have taught an idealistic doctrine. But his chief contribution was to show that no reason could be given for the realistic belief nor for many other beliefs that the rationalist school believed in. He not only pointed out that on the psychological assumptions of his day there was no ground for the belief in enduring physical things, but also that there was equally no ground for the belief in an enduring 'self' or 'ego'—apart from the perceptions or ideas and impressions which are experienced. He thus represented the mind as a bundle of perceptions. It then became very difficult to account for the obvious unity and organization which we find, at least in adult experience, if it is due neither to the thing in which qualities were formerly supposed to 'inhere' nor to the mind which observes and thinks about them.

It would be absurd to attempt to give in a brief review any adequate account of the contribution of Kant to these questions, especially as philosophers are not agreed about its interpretation. But it seems right to point out that in spite of the way Kant is sometimes laughed at now it will still be admitted that he brought to the task a far deeper penetration of the difficulties involved than any of his immediate predecessors. He was not content to score a theological victory with Berkeley, nor was he satisfied with the sceptical makeshifts with which Hume—and still more his later disciples—put difficulties on one side. Kant taught that while knowledge was confined to phenomena, these were, at the same

time, phenomena of independently real external things in themselves which were otherwise inaccessible to knowledge. What was given through sense he supposed to be a chaotic formless manifold which was synthesized by a 'blind faculty of the imagination' into objects in space and time, and these objects were then thought by the understanding, and subsumed under certain a priori general concepts or categories, such as unity and diversity, substance and attribute, cause and effect, etc. He thus ascribed the organization of experience to the activity of the mind, and at the same time insisted that all this unity applied not to things in themselves, but only to their phenomena.

These few remarks will suffice to show the sources from which phenomenalism has sprung. Its fundamental assumption has been that 'sensations', 'impressions', 'ideas', or whatever they are called, are either all that exists or all that can be known. Hence it was concluded that any assertion about anything else is either purely fictitious, and only made in the interests of economy, or is 'metaphysical' and idle. The 'unity of experience' is thus either altogether illusory or is purely the product of the individual mind, and has no objective validity, i.e. it owes nothing to a real world of things and a real organization among them. A curious inconsistency of many phenomenalists may be pointed out in this connexion. 'Organization due to the mind' tends to be identified with 'organization due to the nervous system'. But according to Pearson the nervous system is a 'construct' from sense impressions occurring in human minds. He insists that we must not make ontological assertions about anything but sense-impressions, and then goes on to say that the reason why all people think alike and make approximately the same 'constructs' is that their brains all work in much the same way. So that first the organization of experience is said to be the work of the mind, and then it is declared to be the result of the structure of the nervous system. But how can this be so unless it is interpreted as applying to a real brain, not to a fictitious construct of sense-impressions? This seems to be but another instance of the phenomenalist's habit of running with the hare and hunting with the hounds.

A further assumption underlying all these difficulties which has been taken over from tradition is the belief that sense-impressions are isolated entities with no intrinsic connexion

with one another. It was this belief, coupled with the fact that in adult experience there is no such isolation, which led to the assumption that the connexion must be the work of the mind, and was accounted for by Hume and later empiricists as a result of 'habit' or mechanical 'association'. The blessed word association was made to cover up a great many difficulties which, if they had been frankly acknowledged, might have led to a re-examination of the basal assumptions. It was just because Kant saw that these devices were insufficient that he was led to investigate the question again, but his own training was not favourable to inducing him to criticize the original assumptions which had led to idealism. The difficulties which exercised Kant so much are merely left on one side by the phenomenalists, either because they do not appreciate them, or because they are content to remain in blank subjectivism and to regard all interpretation as illusory.

Having now examined a number of phenomenalistic opinions and learnt something of their historical relations, it now remains to summarize the situation as it stands in their hands, and to explain more exactly in what directions we shall have to look in order to find a better alternative. The root fallacy, as we have plainly seen, is in assuming and making use of the very knowledge the phenomenalists believe to be unattainable. Mach does not begin with the sense organs, but Pearson and Verworn do. Mach and Verworn reach the conclusion that there is nothing but 'elements' or 'sensations'. Pearson says that there is a world of some sort apart from his 'sense-impressions' but that we can know nothing about it apart from its bare existence. Lloyd Morgan also argues from sense organs to the view that the knower is confined to inspecting 'messages' in the 'conning-tower of the bodily ship' but, unlike Pearson, he allows us to know a great deal about what is going on 'in the world of battle' around it. Mach alone professes not to introject, but it seems very probable that he reached his position by means of it. On p. 299 of *The Analysis of Sensations* he states that he 'arrived at views akin to those of Hume' by studying *the physiology of the senses* and by reading Herbart. Thus these writers come to different con-

clusions but they all employ arguments based on the physiology of the sense organs. It is I hope needless to labour this point any further. The sense organs must be treated realistically throughout or subjectivistically throughout. If the term 'sense organ' is only a name for a temporary association of sense-impressions what does it mean to say that sense-impressions in general are dependent upon the sense organs? or that sense organs are stimulated by objects in an external world? Clearly it can only mean that one sense-impression is dependent upon a particular group or groups of other sense-impressions, in the sense that when, for example, I experience the impressions called shutting the eye this is immediately followed by the impression of 'darkness'. According to Mach this is all we should or can mean. It cannot mean that what I see depends on the *existence* of my eye if all that exists are sense-impressions in minds, because then I should see nothing when no one was perceiving my eye, unless with Berkeley we invoke God to perceive our eyes when no one else does.

Let us now turn to the other alternative of treating the sense organs realistically and try to discover what happens in introjection. I think we shall find that the difficulties largely arise from an incautious application of spatial notions and of the category of causation to the cognitive relation. In regard to the spatial question the following remarks of William James are worthy of quotation:

> 'The word sensation, to begin with, is constantly, in psychological literature, used as if it meant one and the same thing with the *physical impression* either in the terminal organs or in the centres, which is its antecedent condition, and this notwithstanding that by sensation we mean a mental, not a physical, fact. But those who expressly mean by it a mental fact still leave it a physical *place*, still think of it as objectively inhabiting the very neural tracts which occasion its appearance when they are excited; and then (going a step further) they think it must *place itself* where *they* place it, or be subjectively sensible of that place as its habitat in the first instance, and afterwards have to be moved so as to appear elsewhere.'[1]

Professor Lloyd Morgan, owing to his adherence to the dogma of the 'double aspect' relation of mind and body (by acknowledgment) is committed to regarding *what is known* as 'inhabiting the very neural tracts which occasion its appearance.'

[1] *Principles of Psychology*, II, 33. The double use of the term sensation to which James refers is still constantly encountered in neurological literature.

And all this may be very interesting by way of metaphysical speculation but it has nothing whatever to do with the theory of knowledge because it is beginning at the wrong end of the stick. On the same page from which the above is taken James adds:

> 'But the supposition that a sensation primitively feels either itself or its object to be in the same place with the brain is absolutely groundless, and neither a priori probability nor facts from experience can be adduced to show that such a deliverance forms any part of the original cognitive function of our sensibility.'

Now the way in which people are led to introject and so to treat the cognitive relation as a spatial one seems to be as follows. When we are studying the relations between physical events, sense organs, and brain, all the entities we are dealing with are on the same level as regards spatial relations. They are all studied by means of visual and tactual perception. To say that a certain object is outside the body and that the brain is inside the skull are at least intelligible statements whatever their final interpretation may be. But when we pass from this to the assertion that what the subject of the experiment senses is in his head and that his act of sensing is also there, then we are entering upon ground to which physiology gives us no warrant and upon which it would be rash to be too confident. It *may* be that acts of sensing (and perhaps sensa) are not 'in space' at all in the sense in which the brain is in the skull. There is no *must* at all about it. For in truth when we are studying physical and physiological events we do not come upon acts of sensing or sensa (i.e. what is sensed) at all 'in' the space in which we find physical events. The only person who can say where his objects are situated, or indeed that they exist at all, is the subject of the experiment, and to him they certainly *appear* to be outside his body, i.e. he is aware of a spatial relation between say, a candle and his body: he can describe the position of the candle with reference to his body or to other things. But he is not usually aware of a spatial relation between his act of sensing and either the candle or his body. I do not see how we can say much about acts or processes of sensing, still less about their spatial relations, and it may be doubted whether they have any, or whether it means anything to say that they have. The persistence of this way of treating the cognitive relation as a spatial one may

be partly a result of the container view of space. If space is regarded as a big box in which everything is contained[1] then it is easy to see how physiologists will be led to treat the cognitive relation in the above way.

If therefore we begin with the phenomenalist's conclusions we are left with the problem of how we come to have the knowledge we appear to have, and if we begin as phenomenalists usually do begin, with the physiology of the sense organs we see that if we adhere strictly to physiological data we do not reach either sensa or acts of sensing at all, and nothing that we learn from the sense organs necessarily compels us to believe either that what we sense or our processes of sensing are spatially localised within the body, but only that our acts of sensing (and perhaps what is sensed) *depend upon* the events in our sensori-motor systems.

But suppose, now, we do assume that what is sensed is situated in the brain and that its characters depend on cerebral events: how, in that case, would it be possible to know that we had sense organs and that external objects were in relation to them? *If* this supposition were true then we must also possess other sources of knowledge, otherwise we could never reach such a belief. If colours, shapes, sounds, etc., are cerebral events of which from time to time I become aware, then my knowledge is obviously not confined to them since by hypothesis I *also* know that they are in a brain and are dependent upon sense organs. I cannot know that I possess a brain at all unless I first know that other people have them. It is only from a knowledge of the anatomy of other men that I reach the belief in my own brain. Clearly then, if my sensa are in my head my knowledge is not confined to them.

The only answer to this difficulty offered by phenomenalists is the theory of projection—with the exception of Mach, who repudiates introjection. But will this really help, does it give a genuine addition to knowledge? Will Pearson's telephone clerk, or Professor Lloyd Morgan's captain in his conning tower, be any better off if they project their messages? If the messages are *given* as internal how do we ever come by the notion of externality, and why should it ever occur to us to project them? If they are given neither as external nor internal why should the lonely and confined contemplators

[1] Prof. Lloyd Morgan repudiates this view. *Op. cit.*, p. 249.

of sense-impressions ever come by the notions of externality and internality? In other words: is there any reason (apart from the necessity of overcoming the consequences of 'introjection') to suppose that such a process as projection takes place *in the absence of any other source of the notion of externality*? Is not Professor Lloyd Morgan simply using his own direct knowledge of spatial relations to help his captain to get out of the conning-tower in which he has confined him? It will help perhaps to understand how introjection comes about if we consider a particular example. Suppose Professor A, the well-known physiologist, is observing his colleague, Professor B, who is looking at a candle. He believes that his friend is surrounded by an impalpable ether which is being set into undulations by the lighted candle. In imagination he follows these undulations until they impinge upon his friend's retina, and there set going another disturbance which is propagated to the occipital lobes of Professor B's brain. So far all goes well because Professor A is dealing with physiological matters. But now he stops talking about physico-chemical changes and motion and begins to talk about sense-impressions. He believes that in consequence of what is happening in his cerebral cortex Professor B now becomes aware of a yellow patch—the flame—and that this patch is in B's head. Professor A thus believes that what B sees is not the candle which he himself is seeing but another candle—a copy which no one but B can see. Thus A follows *in imagination* not only the physical processes which are spatial and can therefore be imagined but also thinks of what B sees and of his seeing in the same way. Professor B, of course, thinks that he is seeing the same candle that Professor A begins by thinking he is seeing. It is clear that B does not introject the candle into his head himself but A does this for him. But if now B does the same thing for A, or if A is of a reflective turn of mind and comes to the conclusion that his own candle is in his own head, then the 'real' candle with which they began becomes rather superfluous, and the two professors may finally come to regard it as a 'fiction'. If, however, they reflect a little further they may also see that in that case retina and occipital lobes are also fictions and the arguments by which they came to their conclusions are also fictitious—unless, as is so often the case, they are among those peculiar people who, as Whitehead says: 'express themselves as though bodies

and brains were the only real things in an entirely imaginary world' and 'treat bodies on objectivist principles, and the rest of the world on subjectivist principles'.

Where Professor A went wrong was in the transition from 'physico-chemical events' in Professor B's brain to the 'sense-impressions' of which B is aware. He did not pause to consider what this transition involved, but in his haste, and on account of his training, continued to think of the sense-impressions in just the same way as the cerebral processes. He forgot that only B has any direct knowledge about such things and that his testimony is the primary source of information about them. Professor A is then liable to treat 'in B's mind' on the same spatial footing as 'in B's brain' assuming that 'in' means spatially 'in' in both cases. Now it seems to be perfectly clear that from the point of view of the theory of knowledge all this is, primarily at least, entirely off the point; because it is trying to do the wrong thing. It is trying to explain *knowing* and it is using knowledge of the external world to do this. Whereas what the theory of knowledge primarily sets out to do is not to explain knowing but to find out what it is that we know. Moreover the introjectionists do not even succeed in explaining knowing. They simply put you and the world that you know into your head still leaving you with the relation of knowing just as it was before, except that they now have the world in duplicate—the world that you know inside your head, and the world that they know and you do not, outside your head. But if they now put the world they know inside their own heads then a common known external world disappears and the only escape from solipsism is the recollection that the whole issue has been begged from the start, and the realization that nothing has been accomplished. When Karl Pearson talks about the knowing ego of each one of us as being seated at the brain terminals of the sensory nerves he is simply putting himself and his world inside his own head in imagination. But this still leaves us with the relation of knowing, and how knowledge is possible is not made one whit more intelligible by bringing what is known into your head. Nothing has been accomplished but a temporary bewilderment which does not really deceive even whose who have been responsible for it.

So much then, for the incautious use of spatial notions in connexion with the relation of knowing. We now have to

consider the use of the category of causation in this relation. It would seem to be evident that caution should be exercised in applying this category in the sphere under consideration. At least it requires some preliminary critical examination of the category itself. But this precaution is omitted by phenomenalists until 'introjection' is completed. Only after they have naïvely employed the category as relating the external object to the percipient do they, as with Pearson and Verworn, come to the conclusion that the category cannot be applied to the external world but only to the interpretation of the subjective world of phenomena. The following appear to be the chief points to be borne in mind in regard to this question.

(a) In the application of the causal category in natural science all the 'objects' are on the same epistemological level. We see one billiard ball run into another. We pull the trigger of a rifle and feel the recoil and see the flash and hear the noise. We are all the time on the sensory level. But with sensing it is different. We see a pin prick a man's skin, and we may call this cause. We see him withdraw the part of his body which is pricked, and we may call this effect. But we do *not* sense the other supposed effects. We do not see or feel either the felt prick or the man's sensing of the prick. Nor is there any reason to suppose that we should have any knowledge of this side of the business even if our sense-knowledge, i.e. natural scientific knowledge, of what goes on in the man's body were enormously extended. We are again confronted with the difficulty of the duality of our ways of knowing already alluded to in the Introduction.[1]

(b) Even when the pin is applied to our own skin the case is not thereby made easier. We see the pin approach and we feel the prick immediately it touches without *perceptible* interval. We sense the 'feel' of the pin and the 'look' of the pin. But we do not sense the process of feeling the prick, nor the process of seeing the look. So true is this that the distinction between *what* is sensed and the process of sensing is not made until we begin to reflect.

(c) But if we recognize this distinction we clearly have a choice of several possibilities in applying the causal category. The physical pin in relation to the physical body may be (1) causally related only to the process of sensing; (2) causally

[1] See above, p. 32.

related to what is sensed; (3) causally related to both what is sensed and the process of sensing.

(d) Now, if what is sensed is causally related to the physical pin so that the latter in some sense 'produces' the felt prick and the seen look, then the consequences of this supposition for the theory of knowledge will be very different from the consequences of supposing that the physical pin only 'calls into activity' the *process* of sensing. The introjectionist often does not explicitly make this distinction, and in any case he treats what is sensed—both the 'feel' and the 'look' as effects of the physical pin (often ending as we have seen *ad nauseam* in denying all knowledge of the latter entity). He sees the pin and treats it first as external cause, then concludes that the seen pin is *effect* and that the physical pin is 'unknowable'. But how in that case can the causal category be applied? How can it be applied at all unless both cause and effect are known? Either the phenomenalist knows the pin or he does not. If what he sees is the pin how can the same seen thing be both external cause and internal effect? As Hume remarked: 'The same principle cannot be both the cause and the effect of another; and this is, perhaps, the only proposition concerning that relation, which is either intuitively or demonstratively certain.' If on the other hand, he does not see (or otherwise become aware of) the physical pin how can he apply the causal category? There seems to be no escape from this dilemma if we treat what we are immediately aware of as the effect of something else as cause, unless we are prepared to admit other sources of knowledge than sense. It may be that what is seen and what is felt is not the pin but what we call the pin as cause is known in some way but not *given* as a sensum.

(e) Dr. Broad has pointed out that if we suppose external objects to set going physiological processes which 'produce' what is sensed then the term production may be interpreted in two ways. Either it means generates or it means selects.[1] But on either alternative we are confronted with a most peculiar type of causation, and still seem to be committed, I think, to all the difficulties of physiological subjectivism.

(f) We might make a distinction between the distance receptors and the 'contact' receptors, supposing that in the former case we were sensing a characteristic of the external

[1] *Scientific Thought*, London, 1923, pp. 323 *et seq.*

object (e.g. the 'look' of the pin) and in the latter a characteristic of our own bodies (e.g. the feel of the pin). This, if I understand him rightly, is the position of Professor Alexander. When we become aware of touches, pressures and organic sensa 'we are contemplating an affection of our own body'[1] but 'we do not see colours in our eyes but only *with* our eyes and in the rose or apple.'[2] We might then interpret the causal category as applying between physical thing and process of sensing, but not between thing and what is sensed. There will be three relations: (1) the causal just mentioned; (2) the characterizing relation between what is sensed and what is thereby qualified or characterized; and (3) the cognitive relation between knowing and the thing known and between sensing and what is sensed as characterizing the thing known. Thus knowing does not consist only in sensing. We shall return to this in the next chapter. The unsympathetic critic might, of course, object that it was mere favouritism to distinguish thus between the eye and internal sense organ. If in the latter case we regard ourselves as being aware of some affection of the internal organs why in the former case do we not admit that we are aware of some affection of the eye?

(g) It must be remembered that if we regard space and time as *sensed*, and treat them in the same way as colour, etc., as physiological events, or as the mental 'other aspect' or 'correlate' of such events, as Professor Lloyd Morgan appears to do, or as 'elements' or 'sensations' as Mach does, then there appears to be no escape from complete subjectivism, whether we acknowledge an external world or not.

(h) Another alternative would appear to be as follows: We could grant the physiological interpretation in the case of the secondary qualities—regarding them all not indeed as mental (since modern thought sees no necessity in this assumption) but as physiological, and appeal to some other process for the direct apprehension of spatio-temporal relations. We should then get the required 'externality' and a spatio-temporal frame independently of the secondary qualities, and we should then have to try to explain how the latter came to acquire a relation to space-time. This is the position of Professor N. K. Smith.[3] He agrees with Professor Alexander

[1] *Space, Time, and Deity*. London, 1920. II., 172.
[2] *Ibid.*, II., 140.
[3] See his book cited above on p. 118.

in believing that space-time is apprehended in a different way from sense, but he regards all sense as physiological. Professor Whitehead's views are in many important respects extremely original and consequently very difficult to fit into relation to other less revolutionary views. In his earlier works he recognised two primary constituents of nature: space-time events, and what he calls 'objects', the latter having the relation he calls 'situation'[1] to the former. There are many different kinds of objects having different relations to one another and to events. In reference to our present problem —the use of the causal category in relation to knowing—he has written: 'The search for a cause of perception raises a problem which is probably meaningless and certainly insoluble.'[2] He certainly believes in a direct apprehension of events as well as of the 'objects' related to them. In his last book he speaks of sense-data (formerly called sense-objects or eternal objects) as 'dependent on the immediate states of relevant parts of our own bodies. Physiology establishes this latter fact conclusively . . .'[3] We shall return to his theories later since they are of great interest and importance for biology.

So much, then, for the present regarding the application of the causal category to the problem of sense-experience and cognition generally. More remains to be said about the general characteristics of phenomenalism. The whole trend of thought of this doctrine appears to be overshadowed by certain buried 'demands' which have arisen with the traditions of physical science, and which prevent phenomenalists from looking with unfettered minds at the facts of knowledge. It is an example of the blinding influence of unconscious naturalistic metaphysics. One factor seems to be the belief that there is nothing 'mysterious' about the cognitive function if what is known is in our skulls and if we are confined to sensing events in our brains. Whereas if we were capable of becoming aware of events going on outside our bodies, this, it seems to be believed, would be a highly mysterious process, and for that reason scientifically disreputable and at all costs to be denied. But what is meant by calling something mysterious? Nothing more, apparently, than that the process in question is not comparable to anything else with which we are more

[1] Or, more generally, 'ingression.'
[2] *Principle of Relativity*, Cambridge, 1922, p. 73.
[3] *Symbolism*, Cambridge, 1928, p. 18.

familiar. But why in heaven's name *should* everything be comparable to what is familiar? If we insist that it must be so how is any scientific or philosophical progress possible? The 'familiar' is, of course, no fixed point, but changes with the movements of thought. Neither does it, as we have seen, make the process of knowing one whit more intelligible to suppose it to take place in our skulls and to confine it to a contemplation of events going on there. It may certainly seem more comforting to think of the process happening in our skulls; it is less uncanny than supposing it to take place nowhere. But only metaphysics needs to concern itself with questions of the nature and spatial relations of processes of knowing. The theory of knowledge is primarily concerned with what is known. And 'familiarity' has nothing to do with the truth. But does it not appear to be the one primary characteristic of the process of knowing that through it we become aware of something other than ourselves? This is surely one of the most familiar of processes and is only *made* mysterious by talking what seems to be nonsense about it and so depriving it of this characteristic. The difficulties would appear to be created very largely by our inveterate habits of thinking with spatial images, and of trying to describe every occurrence in such terms. A glance over the history of physical science shows how scientific thought has been gradually but reluctantly compelled to abandon this procedure. Heat and electricity were at first thought of as 'subtle fluids', i.e. as things or Aristotelian substances. The ether was conceived as a fluid, a gas or a jelly and so on. All these facts testify to the power of common-sense ways of thinking and to the reluctance with which they are abandoned. When men of science turn their attention to epistemological questions, which belong to the philosophical sciences, they are apt to bring with them to the task the same ways of thinking as are appropriate to the natural sciences. Mind is then thought of as a substance with spatial relations.

Common sense is the way of thinking of unreflective thought which, owing to its utility for the affairs of daily life, has persisted and will presumably always persist in its proper sphere. It is the metaphysics of the stone age, and in spite of our modern mechanical inventions our daily life is not so very different from that of the philosophers of the stone age as to necessitate a complete abandonment of their way of

thinking. It is still a perfectly adequate mode of orientation towards the 'macroscopic' world of every-day things. To criticise it in its own sphere would be as absurd as to expect it to be of universal application. For by what heaven-sent dispensation can it claim to provide an infallible guide in the more recondite regions of experience to which the physical and other sciences reach in their developed forms? What confidence can we repose in it for an understanding of vital and mental processes for which it was never devised? The further we depart from common-sense experiences the further we may expect to find it necessary to depart from common-sense ways of thought. Lotze speaks of common sense as 'A faculty which, versed in the criticism of the course of events in the outward world, imagines that the very respectable and probable but quite unsystematic maxims there acquired are sufficient to meet all emergencies.'[1] And Professor Enriques remarks that the appeal to common sense is a 'prudent safeguard for whoever wants to spare himself the critical study of scientific expressions.'[2] Common-sense knowledge possesses, as Mr. Bertrand Russell points out, three chief defects: 'It is cocksure, vague, and self-contradictory'. These three defects are carried over into science, which only gradually frees itself from them as it develops. This we see very clearly from a comparison of biology with physics at the present day. 'The first step towards philosophy,' adds Mr. Russell,' consists in becoming aware of these defects, not in order to rest content with a lazy scepticism, but in order to substitute an amended kind of knowledge which shall be tentative, precise, and self-consistent. . . Philosophy involves a criticism of scientific knowledge, not from a point of view ultimately different from that of science, but from a point of view less concerned with details and more concerned with the harmony of the whole body of special sciences.'[3]

It is this clinging to common-sense ways of thinking which makes the new ways necessitated by relativity and the quantum phenomena so repugnant to many people. These things do not fit in with our accepted 'demands.' But this feature of modern science is by no means confined to mathematical physics, so that it is no longer possible to say that science is

[1] *Logic*, Eng. trans., Oxford, 1888, Vol. I., p. 248.
[2] *Problems of Science*, Eng. trans., Chicago and L
[3] *Outline of Philosophy*, London, 1927, pp. 1-2.

nothing but trained and organized common sense. It would be as true to say that an army is only a trained and organized rabble. In both cases the training and organizing process has been carried to such a pitch that the original is no longer recognizable in the product. But in the case of science the transition has not been a simple continuous process of development, but now involves a drastic overhaul of the very starting-point and foundations.

The further influence of unconscious metaphysical factors as operative in creating difficulties in regard to the problem of knowledge is shown by our persistent refusal to take mind seriously. We do not hesitate to believe the most remarkable things about what we regard as pieces of matter. Most biologists believe without hesitation that at a remote epoch of the world's history matter synthesized itself into living organisms —even in spite of the absence of evidence from experience. The acceptance of this belief does not place the slightest strain on our credulity. But knowing—mind doing its job properly—seems to be regarded as an unbelievable miracle. Rather than admit it to be possible we are prepared to renounce its fruits and treat all intellectual effort as vain and the pursuit of truth as a chimera. This is simply because we are all brought up in the ways of thinking of common sense and the physical sciences, both of which are primarily concerned with bits of matter and take knowing for granted. Moreover, most men of science entertain uncritically a naïve naturalistic metaphysics. We accordingly expect everything in heaven and earth to conform to this or be damned. Thus the nineteenth century cheerfully endured the intolerable and scandalous contradiction of a thorough-going materialistic metaphysic with an equally thorough-going subjectivist epistemology. The phenomenalists are all materialists at heart. Even when, as with Verworn, they profess an idealistic metaphysics they cling without thought of criticism to a methodological materialism. The same factors account for the popularity of epiphenomenalism and psycho-physical parallelism during the last century. The controversies which raged round these questions during the nineteenth century are already beginning to acquire a somewhat old-fashioned air, but the frame of mind which provoked them and was satisfied with the 'solutions' that that age offered is still prevalent, but more from the sheer inertia of the average level of thought than from a deliberately reflective attitude.

In conclusion, then, it seems that the following precautions are necessary if we are to make a fresh unprejudiced beginning in the study of this most perplexing but quite fundamental problem of the assessment of our knowledge.

(a) To avoid as far as possible mixing up metaphysical questions—and still more metaphysical *motives*—with epistemological ones. Mach, as we have seen, places the desire for a monistic metaphysics at the basis of his theory of knowledge. He confesses that it was the mental discomfort occasioned by dualism which first led him to the question of knowledge. In the same way Professor Lloyd Morgan has a metaphysical theory of the relation of mind and body of the 'double-aspect-correlation' type and this enters into his theory of knowledge. But to do this appears to me to be a fundamental error of method. To put metaphysics first, and epistemology second, especially when your epistemology will react on your empirical science, is to put the cart before the horse, and to give a bias to the whole future course of thought.

(b) To avoid the incautious and uncritical application of spatial notions and thinking in spatial images, as well as the category of causation to the relation of knowing.

(c) To avoid as far as possible such expressions as 'mental' and 'physical'; 'real' and 'unreal'; 'external' and 'internal'; 'thing' and 'attribute'. These expressions clearly presuppose an amount of systematized knowledge and the existence of certain specific theories, which we *may* finally come to, but which we cannot simply take for granted at the start, if we are to take epistemology seriously as a sincere critical examination of knowledge.

The only assumption which we must make is the realist one that a common world of some sort exists independently of whether you or I are perceiving it. It is absurd to pretend to doubt this when in fact everyone, whatever his theoretical opinions, does in fact believe it. We shall not then make the absurd mistake of beginning by assuming it and ending by denying it. Thus, as Professor Stout says, the philosopher 'can only start like other people on the basis of ordinary experience . . . We may not ask: Is there an external world? But we may ask: *What* is the external world, and how do we know it?'[1] And to this I would add that we must be careful

[1] *Things and Sensations*: Proc. Brit. Acad., London, 1909, p. 1.

not to confuse the question: How do we know it? with what must, I think, always be the prior question: What do we primarily know of it? which, again, is Mill's question (as I understand it): What are the facts which are the objects of intuition or consciousness? And that this is the prior question becomes clear when we reflect that only in terms of our answer to it can we give any answer to the first. Phenomenalism would not be so popular if this simple consideration were more widely understood.

CHAPTER II

An Alternative to Phenomenalism

It is not to be expected that any 'solution' (in the scientific sense of the term) of what I have been calling 'Mill's question' is possible. We can, as I have already said, distinguish two aspects of this question: (1) What *are* the 'objects of consciousness' in the metaphysical sense, e.g. such questions as: Are they mental or physical or neutral? and (2) What is the relation of these objects to other entities which we are supposed to know? It is our duty from the epistemological point of view to try to be clear about the question in its second aspect, and it may be that in any case the most we can do is to explore the possible alternatives without being able to determine which is *the* solution.

Why, then, it may be asked, need we trouble ourselves about a complicated and difficult question which is admitted to be insoluble? The answer is that, in the first place, thought cannot work in a vacuum—it requires a framework on the basis of which the results of scientific or any other intellectual activity may be expressed. In the second place, everyone does in fact hold some sort of a theory in regard to Mill's question whether it be deliberately thought out or merely taken on trust from tradition. The doctrine of representative perception—aspects of which we have now discussed—used to be regarded as *the* solution; just as formerly the Euclidean geometry used to be regarded as *the* geometry. It is a characteristic of modern thought that it has discovered the existence of possible alternative interpretations in many spheres. Philosophy explores the realm of these possibilities. Science is a technique for determining if possible which of these alternatives is actualised. Some problems can be solved in the scientific sense. If the problem is to determine how many segments an earthworm regenerates if a given number are cut

off this problem can be solved. But some problems cannot be solved in this way, and yet they may be of fundamental importance. The problems of epistemology appear to be of this kind. Whatever alternative we adopt will profoundly affect our interpretations in natural science as will now be evident from the preceding discussions. What we must try to do is to find a theory of knowledge which is self-consistent. The various phenomenalistic theories are rejected because they do not do this. They do not show how one part of our knowledge is consistent with another. In order to keep one part phenomenalism represents another (and that the larger part) as fictitious. Its conclusions, as we have seen, contradict its premises, and therefore its scepticism does not need to be taken seriously.

There are, as we have seen, two dangers to which we are exposed. In the first place we must not allow our epistemology to be constructed with an eye to some particular metaphysical scheme. There is no reason to suppose that the world was invented to suit the conveniences of metaphysicians. Metaphysics is an ideal, a remote possibility, a dream which may some day come true.[1] A theory of knowledge is an urgent necessity without which no metaphysical interpretation is possible. We cannot, therefore, make our theory of knowledge to fit a given metaphysical scheme. The second danger consists in confusing epistemology with other sciences, especially psychology. Epistemology is not primarily concerned with explaining perception—that is a psychological or psychophysiological question. Epistemology is concerned with the unprejudiced analysis of knowledge itself—not with mental or physiological processes. Its task is the assessment of knowledge not the use of knowledge. But it has to give some account of the fact that we believe ourselves to possess a large amount of apparently reliable knowledge of an external world which we all believe to exist and which we appear intuitively to apprehend. It also has to give an account of the existence of both truth and error. In what follows I shall make a tentative attempt to do this in a more satisfactory way than is the case with the phenomenalistic doctrines. But what is here offered does not profess to be in any way a final 'solution', nor to exclude other possible interpretations.

[1] i.e., synthetic metaphysics; *cf.* p. 30, above.

It is first necessary to come to terms with the expression 'real', and to remove certain misunderstandings attaching to the scientific use of that term. Common sense makes a useful distinction between 'appearance' and 'reality' but it must not be supposed from this that appearances are in no sense real. A common scientific notion of reality may be illustrated by the following quotation from Professor Soddy:

> 'The modern habit of thought recognises things as having a real existence apart altogether from the particular qualities or properties by means of which the things make themselves known to the five senses. The acceptance of this habit of thought among scientific men has been due mainly, not to any factual proof, but to its fertility and to the undoubted value of the results which follow from it. Deep down somewhere in the processes of thought the ultimate test of reality appears to be the Law of Conservation. Does the soul exist? If so, it must be immortal. Is matter real or a mere impression of the mind? It cannot be created or destroyed, and therefore has an existence apart from the mind. Lastly, has energy a specific existence, or is it merely a convenient abstraction? Energy is conserved like matter, and therefore obeys this test of objective existence.'[1]

There are many interesting epistemological points raised in this passage, but for the present we must concentrate on the question of reality. It is evident that Professor Soddy is making a distinction between 'real' and 'mere impression of the mind'. He limits the application of the term real to that which is conserved. But apart from the difficulty of knowing whether anything—even matter and energy—is absolutely conserved (especially in view of the modern attitude towards the conservation of physical entities) it is surely an undue restriction to limit the term real to a particular type of entity in this way. When I have toothache the pain is real in *some* sense at least while I have it. It would be cold comfort to be told it was unreal—either by a physicist or by a Christian Scientist who would both apparently agree on this point however much they differed on others. If all 'mere impressions of the mind' by which matter and energy 'make themselves known to the five senses' are unreal we are committed to the belief that our knowledge of the real is solely acquired by an inspection of the unreal. Notice the complete

[1] *Matter and Energy*, London (undated), pp. 40-1.

inversion of the phenomenalistic position. For the phenomenalist the 'impressions' are the only real, or at least the primary real, and Professor Soddy's matter and energy are only 'convenient abstractions'.[1]

We want, therefore, at the outset to use the term 'real', if we use it at all, in a way which will be independent of the above two opposed views if possible. The only proper procedure would seem to be to use the term real for anything which may be said to exist. A toothache exists at least while someone is aware of it. One cannot be sensuously aware of the non-existent. If someone is aware of it it has *some* sort of existence although it may well be a sort of existence in which a physicist would not be interested. He would not call it *physically* real, just as Galileo did not regard the secondary qualities as physically real. But whatever adjective we may choose to attach to them they are in some sense real and we cannot limit the term to the things physicists happen to be interested in. It will be better, therefore, to avoid special adjectives at first and to use the term real in the widest possible sense as equivalent to existent. A toothache need not be immortal in order to exist, and in that sense be real, as Professor Soddy would appear to suggest.

Let us therefore call all that exists the world of being—using the term as a mere descriptive title—without stopping to make reservations in regard to permanent or fleeting existence or to the difference between 'being' and 'becoming'. Neither need we make here any assumptions about whether this 'world' is a unity or a collection of unrelated or loosely related entities. Let us try to make a bare list of what we appear to find without any metaphysical implications about their relative worth or degree (if there are degrees) of 'reality'. We can call the major items in our inventory 'realms'. The difficulty will be to find names for these realms which do not have implications and associations of the very kind we wish to avoid. First and foremost we can distinguish that realm which includes Mill's 'objects of intuition or consciousness'. It is the realm which 'contains' coloured

[1] Note the erroneous antithesis between 'abstract' and 'real.'

patches, smells, feels, sounds, pressures, temperatures, etc. It is the realm of bare awareness—a realm about which it is so difficult to speak and about which we know so little, because it is not the world of daily life. The latter is the realm of *things*. Common sense passes at once from the world of which I am speaking to the realm of things. When I say that the realm in question 'contains' colours, etc., I do not mean that these are 'distinct existences' as Hume assumed. They are singled out from the realm in which they are embedded and can then be talked about. But I am not speaking of the talked about colours, etc., but of these colours as immediately experienced or sensed, before they become *objects* of thought. I shall contend that primarily this realm is not known but only, as the Germans say, *erlebt*. It is the realm of *Erleben*, which is an 'activity' common to all conscious life, whereas knowing is only one among many activities. Moreover it will generally be admitted that this realm of bare awareness is the starting point of all conscious human activity. Whether our standpoint and interests are in the direction of science, philosophy, art, religion, or ordinary practical life, this is the realm from which we begin and to which all other realms we may discover in some way ultimately refer. It is the realm which is primarily given. We do not consciously contribute anything to it, we only select from it. The other realms which we discover are the outcome of the pursuit of a particular interest or standpoint towards this primary realm. I shall speak of it as the Primary Realm in order to emphasize this fact and in order to avoid dividing it up into mental and physical.

Now although the primary realm is the actual starting point for all conscious human activities it is not the realm from which we deliberately and consciously begin. The realm from which we uncritically believe we start is the realm of common sense. But this is already a realm of knowledge—a realm reached by the exercise of the cognitive or intellectual activities. This we take at first at its face value until we begin to reflect. It is only after this critical reflective process has been pursued that we discover the primary realm which has long been passed beyond for the purposes of every-day life. The primary realm is too intolerably complex for common-sense purposes and the common-sense thing represents a step in the direction of simplification or abstraction. Later the realm of knowledge

is deliberately extended as a good or end in itself: we deliberately seek to analyse and discover relations—the two fundamental characteristics of the reflective or intellectual attitude—and we then enter the realm of scientific and philosophical knowledge. The chief difference between the scientific and the philosophical attitudes is that science begins with the realm of common-sense knowledge and extends and corrects it, whereas the philosophical standpoint deliberately aims at beginning 'below' or 'before' the common-sense level, i.e. it seeks to get back to the primary realm.[1] But, as we have seen, science in the course of its development, also finds it necessary to take up this attitude and the difference between science and philosophy begins, from this point of view, to wear a little thin.

Similarly the artist seeks to get back to the primary realm, to recover 'the original innocence of the eye' and his work is the outcome of the aesthetic activity, which is not primarily an intellectual one. He does not deal with common-sense things, which are products of intellectual activity. It is for that reason that people who are not artists expect art to depict the likeness of common-sense things, and are disappointed when this is not done. These facts are also illustrated, I think, by the story of van Gogh who came home hungry one night to find two herrings on his plate for supper. But instead of apprehending them as eatable herrings as a 'normal' person would, he was seized with an aesthetic inspiration and painted a picture instead. My view, then, is that knowledge at all levels is the outcome of the intellectual activity and objects or things belong to the realm of knowledge not to the primary realm. The objects of knowledge are in a sense (which must be carefully explained)[2] creations of the intellectual activity just as much as the objects of art are creations of the aesthetic activity. But in neither case are they *free* creations—there are 'canons' of art and of knowledge. The task of epistemology is to study the relations between the realm of knowledge and the primary realm.

If this is correct—if the realm of common-sense things is a realm of knowledge and not the primary realm—then it

[1] Cf. S. Alexander: 'The duty of the philosopher is to put himself into the skin of the innocent original.' *Mind*, N.S., XXXII., 4. How far it is possible to do this we shall see.

[2] See below, p. 147

follows that perceptual objects—the tables and chairs of every-day life as well as the animals and plants of biological laboratories—are also thought-objects not entities existing as such in the primary realm. The ease with which this is forgotten is simply a consequence of the immediacy with which the process of perceiving takes place in adult life. We acquire an unconscious facility in perceiving things, just as we acquire an unconscious facility in riding a bicycle, although both have to be consciously learnt. Perceiving things is not simply sensing but *thinking* or knowing. Occasionally we have experiences which remind us of the fallibility of this 'automatic thinking'. I once left a crumpled-up furry glove on a shelf on which I do not usually place gloves. Coming into the room one day I saw an animal on the shelf—a furry animal. Not expecting to find one there, and wondering what sort of animal it was I investigated it further. During this period I was *seeing* an animal—my percept *was* an animal. But with further investigation this object soon vanished. As I looked my percept changed. The experience is similar to watching a 'dissolving view' but it takes place with great rapidity. The animal gave way to a glove. I remembered putting it on the shelf, and my attention was then free to turn to other things. Thus the primary realm can be interpreted in more ways than one although some interpretations endure and others do not. But if I am to see things it *must* be interpreted.

Now consider a beginner looking through a microscope for the first time at a histological preparation. If he has seen no books and has not been told 'what to look for' he sees very little, as his teacher quickly discovers if he asks the student to make a drawing. The drawing will be a confused blur of colour—highly 'impressionistic' (even supposing that difficulties of focussing are excluded). The sharply distinguished parts which the teacher sees will not be discoverable, because they are not *objects* for the pupil. He does not perceive them for that reason. The student is, as it were, 'born again' through the microscope. He is an infant in the microscopical world. He cannot perceive things in that world because he does not possess the requisite thought-objects. What is presented to him through the microscope is part of the primary realm and things belong to the realm of knowledge by means of which the primary realm is interpreted. His teacher, on

the other hand, has lived longer in the microscopical world and has developed the requisite thoughts and therefore sees, not a confused blur but sharply defined objects. Moreover he knows what to neglect and his drawing will therefore be simpler than the beginner's which will depict without discrimination. Every teacher of microscopical anatomy will bear witness to this fact and also to the way in which beginners do not see constituents of a preparation which to him are 'really there'. What to the pupil is a confusion in a wider confusion and scarcely if at all discriminated from it, is for him a section of a blood-vessel related in a definite way to other tissues or organs.

I conclude, therefore, that the awareness of the primary realm excites in us a process, primarily intellectual, however automatic it may become, which results in our seeing a thing or object. We do not sense the object. We sense the primary realm but we perceive the object. We know the object but we do not know the primary realm save by means of the object. It is in terms of the object that the primary realm is known. This is in agreement with the fact that common sense never stops at the primary realm but passes at once to things, and also with the fact that common sense does not classify the ingredients of its realm by colour or sound, or feel or even shape, but by what things do or what they are for. The classification is into sorts of things—animals and plants, chairs, houses, stones, tools, etc. The primary realm is not even noticed if it cannot be immediately interpreted in terms of things. It is notoriously easy to 'miss' an unexpected or unfamiliar element under the microscope.

We must attend a little at this stage to the interpretative process. Finding myself in a room with a strange and complex wall-paper I begin, in the absence of anything better to do, to analyse the pattern. The eye runs over the whole surface in the attempt to discover the 'unit' which the pattern repeats. Having discriminated it I proceed to analyse it into its parts and their relations. When this is done and it is possible to grasp the unit of the pattern the whole expanse is 'understood', i.e. it is mastered by thought. Any part of the wall-paper pattern can now be 'explained', i.e. assigned its place in a system. This whole procedure is characteristic of the mind's working in its functioning in the service of knowledge. Always something is given or presented—an 'occasion' of

awareness. This is complex and to be grasped it must be analysed. It must be deprived of its complexity, if possible by the discovery of something which is repeated or exemplified many times. Hence the popularity and value of 'atomistic' notions in science. The microscopical field is a confused blur to the beginner partly because he does not attend to it sufficiently closely, and it is difficult to attend to because it contains no objects for him. His mind finds no foothold in it. Analyse it how he may the products of analysis at first run together like drops of mercury for lack of anything to keep them apart. He says he can make nothing out of it and the expression 'make' is significant. It implies that objects do not simply reveal themselves but have to be dug out by an active analytical process on his part. No progress can be made until something is discriminated which recurs and can be *recognized* as being exemplified many times. This is the function of things. They form centres round which the recurrent features of the primary realm crystallize while the non-recurrent features pass on unnoticed. A given microscopical field is unique, and, as such, thought can do nothing with it. Thought works with concepts or universals, and what the beginner in microscopy does is to discover the enduring or pervasive features of the microscopic world and retain them in the world of knowledge as perceptible *things*. Now the same thing happened with the early microscopists. They had neither previous microscopical experience nor books to guide them. They must therefore have proceeded by analysis and comparison and thus created a basis for further interpretation. We all know how long it took to develop the concept of the cell, and what different forms it assumed. But when once it was elaborated it provided the key to the interpretation of the microscopical world in much the same way as the discovery of the repeat provides the key to the interpretation of the wall-paper pattern.

I conclude, therefore, that common-sense things or perceptual objects are concepts or universals in the above sense, and that they are reached by a process of analysis and synthesis of what is presented by the primary realm. Moreover, when we are said to be perceiving a thing we are always perceiving a universal, not a particular.[1] But the primary realm

[1] All that I mean by a 'universal' here is something which is 'repeatable' or recurrent, and hence 'recognizable,' in other words an 'object' in Whitehead's most general sense.

may be interpreted by different percipients on the same occasion as an object of different degrees of generality. As Lossky says, we ordinarily perceive objects of a medium degree of generality.[1] For example, a savage might not perceive a chair at all, he might not even discriminate it as a 'thing'. Or, on a given occasion of perceiving he or a European might perceive it only as an obstacle to be avoided in walking. But if our purpose is to sit down we would perceive it as a chair in general, or, if there is a choice of chairs we might particularize further and perceive it as a comfortable chair. Whilst a connoisseur would again particularize further still and perceive say, a Chippendale chair. But in all cases, it would be a universal and not a particular which is perceived. It would only be particular in the sense of one and not many chairs, but this again is to perceive it as exemplifying the universal 'one.' What is a particular never to be repeated is the actual occasion of perceiving in question. If the particularizing process is pushed far enough we come to a bare description of a region of the primary realm on that occasion, and this would be of little or no value for knowledge.

Common-sense things or perceptual objects are what the primary realm is *known as*. Perceiving is a mode of knowing of a somewhat primitive type, and perceptual knowledge is limited in scope. A given white powder may be indistinguishable by mere perceptual inspection from many other white powders. That is to say many different regions of the primary realm may be known perceptually both by a housewife and by a chemist, as 'white powder.' But to both it may be known as more than that. To the housewife it may be known as common salt and an ingredient for cooking, while to the chemist it may be known as NaCl. What does this mean? It cannot be known as NaCl in bare perceptual inspection. This involves something we have not so far considered, namely what are called properties of things. This will be discussed in the next chapter.[2] A white powder can only be known as NaCl by observing what it does in relation to other things, whereas so far we have only been considering its relation to percipients. As a further example consider a rabbit. This is known as a perceptual thing by a cook, a bio-chemist, a physiologist, and an anatomist. But to each one it will be

[1] *The Intuitive Basis of Knowledge*, London, 1910, p. 295.
[2] See below, p. 178 ff. (Sect. 3).

known as more, but the more will be different in each case. The bio-chemist knows the rabbit in terms of chemical composition, the physiologist knows it in terms of parts exhibiting functions, and the anatomist knows it as a system of parts in spatial relations of a certain kind. Thus in one sense the object of study is different in the three cases depending on different modes of abstraction, and in another sense it is the same. Note that the anatomist's knowledge is still perceptual knowledge derived from a further analytical discrimination of parts in the one thing. The objects of the physiologist and still more of the bio-chemist are not of this nature but resemble rather the NaCl of our former example. Thus in all cases so far considered the primary realm offers the starting point for thought and the first step is the discrimination of things which are susceptible of progressive specification but can never be recognized or entertained in thought except as universals. The notion of properties of things arises from the observation of their relations to other things. And just as we tend to think of the things not as objects of knowledge but as actual constituents of the primary realm so we tend to think of the properties as in some way "rolled up" inside the things.

If the above account is correct—that perceptual objects are not simply found as such in the primary realm then the question arises whether it is possible to say anything about the bare primary realm. Some of the upholders of the sensum theory express themselves as though they believed that what they call a sensum (e.g. a coloured patch) is a part of the bare given divested of all interpretation. But in contemplating a sensum are we not substituting one interpretation —one *object*—for another? Is not a sensum just as much an object—what the primary realm is known as—as an ordinary percept? My first percept in the case of the glove was a sensum percept of an animal, but it did not endure but was replaced by the percept 'glove.' In the same way sensa do not persist but give way to ordinary percepts. What I see when I look obliquely at a penny is, according to the sensum theory, an elliptical object. This appears to be regarded as primitive—the very primary realm itself. But is it not as

much an interpretation to say: There is an elliptical flat object, as to say: There is a 'round' object with one edge further away from my body than the other? It seems possible that the sensum-theorist's object can no more be regarded as divested of interpretation than the plain man's. And it is this impossibility of suppressing the interpretative processes which seems to render any attempt to characterize the primary realm as such so difficult. A new-born infant may possess the information we require but unfortunately cannot communicate it. The beginner in microscopy comes somewhere near it so far as the microscopical world is concerned, but he is already equipped with knowledge of spatial relations acquired in the macroscopic world.

If sensa are *not* genuine 'raw materials' any more than common-sense things why not begin our analysis with the latter? They have the advantage of being in some sense public whereas sensa are supposed to be private, and at least on the common-sense level (on which in ordinary life we all live) there is a good deal of agreement about them, and it is well to begin with agreement. Moreover we have to remember that our bodies too are common-sense things whatever else they may be. They exist with other things 'in' perceptual space and enter into various other relations with other things. It is hopeless to begin with 'sensations' or sensa and attempt to stick them together into any sort of world. So much depends on your starting point, and if you begin with 'sensations' (sense-impressions) as ordinarily understood, there seems to be no escape from subjectivism. This method has been tried and found wanting. Once and for all Hume has taught us that lesson, and we now know—what Hume did not know—that 'sensations' are not the raw materials they were supposed to be but highly sophisticated products of analysis, only to be found in physiological and psychological laboratories.[1]

Anyone who wishes to study the sensum theory may do so in the able and painstaking discussions of Dr. C. D. Broad.[2] Personally speaking I find it very difficult to believe, and I fail to see how it escapes from the difficulties of the old forms

[1] It is encouraging to find that even neurologists are beginning to uncover this. Cf. Head, *Studies in Neurology*, II., 831.

[2] See his *Perception, Physics, and Reality*, Cambridge, 1914, also *Scientific Thought*, London, 1923, Part II., pp. 227-348, and *The Mind and its Place in Nature*, London, 1925, Chap. IV.

of the doctrine of representative perception and of psychological atomism.

Common sense things or perceptual objects are analysable it seems in two ways: (1) by *manipulation*, and (2) by thought, and the products of analysis are different in the two cases. For example, a wooden box is analysable by manipulation into lid, sides, and bottom, and these are still things or perceptual objects. The box can also be analysed in thought in another way, namely into a volume, various colours, hardness, temperature, textures ('feels') etc. In other words in thought a perceptual object is analysable into a volume and various 'sense-qualities'[1] whereas this cannot be done by manipulation. I cannot by manipulation separate the colour from the volume without also separating a part of the volume. The differences between these different methods of analysis suggest that the relation of volumes to one another is not the same as the relation of volumes to the sense-qualities. This is strengthened by many well-known experiences. A volume of blood may be red to one observer and yellowish to another at the same time. Moreover, the relation of these qualities to volumes differs in the case of different qualities. Sounds and smells, for example, have a different relation to volumes from colours and temperatures. Thus the connexion between volumes and sense-qualities seems to be different from the relation between two volumes, e.g. the lid and the bottom of the box.

Moreover, the relation between two volumes is different from the relation between two sense-qualities. All volumes are related to one another by other volumes, whereas there seems to be no connexion between, say, red and the smell of sulphuretted hydrogen, and if there is it is not another sense-quality. Some subjectivist arguments derive their plausibility from the difficulties attending the relations between volumes and the sense-qualities. But they usually speak not of volumes but of objects. In the case of the blood, for example, it is urged that because two observers sense a different colour in relation to the same blood at the same time the colour must be 'subjective' and the blood not coloured at all. Similarly it is considered strange that a penny should exhibit different shapes from different points of view because the

[1] I am using this term here merely as a descriptive term without any implications regarding relation to sense-organs.

shape of the penny is supposed to be constant. How then do we reach the belief that the 'physical penny' is of constant shape if we are given only its changing appearances? It is only after we have got the notion of the constant object that we regard the varying appearances as strange. And yet it has been through the latter that we have reached the former, and how strange it would be if the shape of the penny did not change as we change our point of view! There seems to be no ground for concluding that 'the senses seem not to give us the truth' about the penny. The terms 'truth' and 'falsehood' do not apply to the primary realm. What the senses 'give us' are the varying shapes which belong to the primary realm, but the penny as an object does not belong to that realm at all but to the realm of knowledge, and it is because it belongs to the realm of knowledge—of interpretations—that it is eternally of one fixed shape. If for a moment we were to borrow the language of the projectionists we might say that we 'project' the penny from the one realm into the other—into the primary realm and then are surprised that it does not always 'fit.' In the same way we 'project' the blood which, as an object, belongs to the realm of knowledge and usually has the predicate red attached to it, into the primary realm and then are surprised when it sometimes is yellow. This is a complete reversal of the usual account of 'projection.' The projectionist thinks that objects are spatially outside his body and their qualities are inside it, and he projects the latter onto the former, usually contriving by some miracle to hit the invisible target. But on the view taken here the primary realm is the realm into which he projects and objects belong to the realm of knowledge which is not a spatial realm at all. This, however, is anticipating somewhat because we have not yet discussed space. If we are to abide loyally by what we find then we must accept as a datum that the primary realm is such that one observer sees red when and where another sees yellow, and one sees 'elliptical' when another sees 'round.' This means that *what* the two observers are looking at is not a thing in which a number of qualities inhere but a region of the primary realm—a volume—to which various qualities (both primary and secondary) are related in a complex way, not merely to that region but to other regions, including what is called the body of the percipient and the intervening medium. This

is of course shocking to common sense because the latter is a most necessary and useful *simplification* of what is the case. Common sense, quite legitimately for its purposes, considers the volume in question and neglects everything else. If by A 'having' the quality B we mean that under all circumstances when we apprehend A we shall apprehend B then the volume cannot be said to have the colour in question. But a volume when visually apprehended is also seen to be related to some colour, but *which* colour depends on the circumstances under which it is seen and no volume is seen out of relation to something else. The same remarks apply to shape. The success of the common-sense habit of simply predicating the colour of the volume depends on the constancy of ordinary circumstances of perception. There is no occasion whatever so far to say that colours are subjective or mental. All we have to admit is that the relations of colours, etc., to volumes is a great deal more complicated than is supposed by common sense and that need not surprise us. Neither need it be concluded that the 'realm of knowledge' is mental. Mental and physical are metaphysical terms to which we have not yet arrived, and it will be well to postpone them as long as possible.

As regards shape, what is called *the* shape is always an interpretation and belongs to the realm of knowledge. It is the one which is usually 'upheld' by further exploration, like the glove in my former example. A small wooden cube on a table, for example, is perceived as a cube from all points of view (provided we are not *too* close to it) but it may be interpreted in other ways, e.g. as having a protruding or a receding edge, etc. But these differing interpretations do not persist in the way that the interpretation called the '*real*' shape does. The shape as seen from a given standpoint is *one* aspect of the volume in question since in a given standpoint we are committed to the 'egocentric predicament'. The so-called 'real shape' as perceived (i.e. interpreted as cubical) is reached by a synthesis and selection of the aspects. Moreover the 'real' shape guides the carpenter in making the cube in such a way that it will yield the various aspects.

So far we have had the primary realm as consisting of primary and secondary qualities related to one another in a complicated way through volumes. All this I take to be primary and given. On the other hand we have the realm of

knowledge which is a world of interpretations reached by the exercise of the intellectual activity directed towards what is given. I have confined attention to what is given in sense-awareness because we are concerned with natural science, not because I suppose that this is all that is given. Something more must be said about volumes and in justification of the inclusion of this element in the given. If it is not so included I am unable to see any escape from subjectivism.

When I perceive a cubical box I am not aware only of *its* volume but always of a wider volume which encloses it and also encloses my body considered as a volume.[1] The same is true if we consider auditory perception. When a bell is rung behind my back I am aware that *something is happening* (i.e. time is involved) in that wider volume, although it is but vaguely specified as regards location and character. Thus one discriminated volume is always discriminated as part of a wider volume, and two volumes are always discriminated as enclosed by a wider volume and as thereby related. Professor Whitehead calls this fundamental relation between volumes the relation of *extension*.[2] He also points out that the same relation holds between periods of time. We perceive volumes during periods of time. One period of time encloses other periods and is itself enclosed by other periods, or overlapped by other periods. Also there may be periods which neither enclose nor overlap a given period. Professor Whitehead shows how the points, instants, lines, etc., of mathematical physics can be derived from volumes during periods of time by his Method of Extensive Abstraction. Now nothing of this kind can be said of the sense-qualities. They have no parts except in so far as they borrow this character from the volumes to which they are related. It should be noted that although in the case of the ringing bell the time element is not easily forgotten, in the case of the visual apprehension of the cube of wood the fact that what is perceived is a volume during a period of time is much more likely to be neglected. But it is evident that the two cases are not different in this important respect.

It seems, then, that what we are aware of is not simply confined to sense-qualities, but also includes volumes during

[1] Cf. N. Kemp Smith, *Prolegomena to an Idealist Theory of Knowledge*, London, 1924, p. 84.

[2] *Principles of Natural Knowledge*, Cambridge, 1919, p. 75.

periods of time.[1] But these must not be confused with the intellectual constructions called space and time which belong to the realm of knowledge. We need to consider the relation of sense-qualities to volumes during periods of time a little further. The difficulties of this problem largely arise, as I have said, from the common-sense habit of thinking in terms of objects *possessing* qualities. And objects really do possess qualities and properties and much else, because that is what they are for. An object is a kind of portmanteau into which we pack our experiences. But objects belong to the realm of knowledge and when we are trying to get back to the primary realm they will only lead to confusion. Consequently we need not expect sense-qualities to have the same relation to volumes during periods of time as they do to objects. I adopt the view of Professor Whitehead that the sense-qualities (or sense-objects as he calls them) have a multiple relation to volumes, and cannot, therefore, be said to be the qualities *of* one volume concerned rather than another. When I am said to be seeing a red brick this may be said to be the colour of the volume which I am knowing as a brick under that particular set of circumstances so long as we remember that it is not 'possessed' by the volume in the same way in which it is possessed by the brick, i.e. in a two-termed relation. It is not a character of the volume in the way that it is a character of the brick. It is a character of the whole complex constituting that particular occasion of perceiving. The brick, on the other hand *is* a character of the volume which is known as a brick. It expresses the more or less permanent but abstract features of the history of the volume in relation to other volumes in the primary realm. For it is the function of objects to gather up and preserve in knowledge the more enduring or recurring features of that realm. Thus although I said that objects

[1] Cf. Whitehead: 'Material bodies only enter my consciousness as a representation of a certain coherence of the sense-objects such as colours, sounds, and touches. But these sense-objects at once proclaim themselves to be adjectives (pseudo-adjectives, according to the previous chapter) of events. It is not mere red that we see, but a red patch in a definite place enduring through a definite time. The red is an adjective of the red time and place. Thus nature appears to us as the continuous passage of instantaneous three-dimensional spatial spreads, the temporal passage adding a fourth dimension. Thus nature is stratified by time. In fact, passage in time is of the essence of nature, and a body is merely the coherence of adjectives qualifying the same route through the four-dimensional space-time of events.' *Principle of Relativity*, p. 54. See below, pp. 181 and 339.

were in a sense creations of the intellectual activity they are not *free* or arbitrary creations but represent at the same time *discoveries* about what is actually the case. They are the result of abstraction and selection and to that extent may be said to be 'free' in so far as we are free to abstract this or that feature. So much then for sense-qualities as terms in a characterizing relation to volumes during periods of time. What can be said about their place? It is first necessary to note that only volumes have spatial relations since every volume is related in a definite way to other volumes by yet other volumes. Sense-qualities do not appear to be entities which are capable of 'simple location.' They are apprehended as related in a vague way to volumes—obviously so in the case of smells and sounds, less so in the case of 'feels' and colours. If we are to put colours anywhere we should put them in the place in which they appear to be, but they are 'there' in a different sense from the sense in which a volume is 'there'. Considering their philosophical history it is not surprising that sense-qualities should fail to find a comfortable place in our ordinary ways of thinking since the latter have deliberately refused to have anything to do with them. Putting them in people's heads neither banishes them from the world of being nor makes their coming and going any more intelligible. We seem to be driven to admit that they are entities of a peculiar kind as regards their spatial relations. They are spatial only in a secondary or derivative sense.

Something must now be said about those situations in which we perceive objects although, as common sense expresses it, there is 'nothing there'. These situations include dreams, hallucinations, and illusory situations in waking life. The characteristic of all these situations is that the objects are private and are not perceived by other percipients, whereas, in normal perceptual situations two or more percipients, however much they may differ in detail about a given object, owing to their different points of view, will usually agree that they are both perceiving the same object and will agree about its general character. This means that they are both knowing the same region of the primary realm as or in terms of the same thing or object which it exemplifies. Moreover, even in the

absence of another percipient we do not normally remain long in doubt about a perceptual object. As in the example already given (p. 136) further exploration either upholds our first interpretation or the latter is replaced by another interpretation, i.e. another object which is upheld. Thus a perceptual object is regarded as 'normal' or 'true' or 'real' when it is perceived by a number of percipients or when it is upheld on further exploration. An 'appearance' is either a bare uninterpreted bit of the primary realm, or it is a bit incorrectly interpreted. It usually means a situation in which there has been a breakdown in the customary process of reducing all experience to the 'familiar.' In non-illusory perceptual situations our bodies are in relation to a certain discriminated volume during a period of time which 'upholds' or 'exemplifies' a universal of a certain kind, namely a common-sense thing or perceptual object. This perceptual object is what that volume is known as. In natural science all knowing is knowing something as something. The verb 'to know' is extremely ambiguous. In one sense we may be said to know the volume, in another sense we only know the perceptual object. The volume and the related sense-qualities and other related volumes during a period of time are apprehended, or intuited, or *wield*, but a particular region is singled out by analytical attention and *known* as an object. 'This is a glove' means 'I am here and now in the presence of a volume there which is now characterized in a certain way which distinguishes it for me from other volumes.' It does not mean, as common sense seems to suppose, 'I am here and a glove is there and that is all *there* is to it.' It means I am here knowing that there *as* a glove. But 'that' may be known as many other things, for example, by the various branches of natural science and by various trades of daily life. 'That' belongs to one realm, and what that is known as belongs to another realm, namely the realm of knowledge. Natural science is a part of knowledge. Its propositions make assertions about 'that' in terms of its various objects, i.e. what it knows 'that' as, and these assertions are upheld by 'that' and are therefore called 'true.' But the realm of natural scientific knowledge is wider than the realm of perceptual knowledge because it contains objects which are not exemplified in the primary realm in the same way as are perceptual objects. But if they are based on experience

they must be exemplified in *some* way. Thus atoms and electrons are no more literally 'in' the primary realm than are chairs, bricks or rabbits. By saying that the atomic theory is true what is meant is that the primary realm upholds or tolerates an atomistic interpretation, just as it upholds the interpretation 'chair' or 'brick', but in a less direct and obvious way. But in all cases what we immediately observe seems to be one volume, discriminated from other volumes, during a period of time, with its related sense-qualities. Now Professor Whitehead calls a volume during a period of time an event and in future it will be more convenient to use this expression. I shall follow Professor Whitehead in what he says about events, but I am doubtful how far my view will agree with his theory of objects. He calls sense-qualities objects. But as sensed they are not objects in my view but constituents of the primary realm. They can become objects as soon as they become concepts. Red as sensed is one thing, but red as known or thought is quite another thing. Also perceptual objects do not, on the view here put forward, belong to the primary realm but only to thought. But that does not mean that they are not (in *some* sense) 'real.'

In spite of repetition it will be desirable now to test the above theory, and at the same time make clearer what is meant, by analysing an actual biological proposition such as a geneticist might enter in his note-book. We can take the following:

> 'This rabbit is white, weighs six pounds and measures 1ft. 9in. from the tip of the snout to the tip of the tail.'

All this seems thoroughly 'objective' and public. No biologist would find anything in it to take exception to. It is what is called a fact. Let us consider the precise meaning of each word.

'This' serves to direct attention to a certain region in the perceptual space of the observers. It refers to a certain spatial volume during a period of time, i.e. to an 'event'. 'Rabbit' indicates that the region in question has been discriminated from other regions and recognized as exemplifying a universal. It is what the event indicated by 'this' is known

as. It is a perceptual object now being exemplified in the experience of the observers. 'Is' means 'is characterized by'. 'White' is another universal, related to the perceptual object 'rabbit' by the characterizing relation, but the white as sensed is an ingredient of the primary realm just as is the event to which it stands in relation. 'Weighs six pounds' means that the operation of weighing has been performed, i.e. that which is known as a rabbit has entered into certain complicated relations with other events with the passage of time with a certain observable result. 'Measures 1 ft. 9 in., etc.', indicates that a certain standard measuring rod has been applied to 'this' in accordance with certain rules, the expression 'from the tip of the snout to the tip of the tail' specifying the direction along which and the limits between which it has been applied.

Now the above propositions presuppose what is called a normal observer and normal conditions of observation. If we introduce a man wearing blue spectacles and a man born blind the situation becomes more complicated but need not land us in scepticism. The man with the spectacles will declare the rabbit to be blue quite correctly because the rabbit he sees is blue just as truly as the rabbit the normal observer sees is white. If each predicates blue or white only of the visual perceptual object he perceives there is no contradiction because there is a two-termed characterizing relation between the perceptual object and the colour, but if each one predicates the colour of the event which is being known as a rabbit then they fall into contradiction, because they have omitted the other terms to which the colour as sensed stands in a multiple relation. The normal observer omits these terms because he relies on the constancy of conditions which enable his assertion to stand as if the colour and the event known as a rabbit were more simply related. To the blind man, on the other hand, the assertion that the rabbit was white would be meaningless because he is incapable of visual perception.

When we turn to the assertions about length and weight there is no reason why, with suitable contrivances, all three observers should not agree. Whether the event be known as a blue rabbit or a white one makes no difference to its length —it even makes no difference whether or no it is known as a rabbit. What is measured is not the rabbit but the spatial separation between two limits of the extent which is knowable

as a rabbit. The rabbit as a perceptual object is a character of that extent, and it is the extent that we measure. These facts illustrate the abstract character of physics and why it is that the 'secondary qualities' came to be considered 'unreal'. But from the biological point of view they may be very important. To a geneticist the fact that the rabbit is white is just as important as its length. A little consideration will show that it is neither the visual rabbit which we see nor the tactual rabbit that we touch which is measured. These only serve to guide us in adjusting the measuring rod to the spatial extent which is measured. In determining the length the rod must be parallel to the direction along which it is applied and the line of sight must be perpendicular to the rod. Thus measuring with a rod presupposes bodily movements through which the requisite congruence relation is brought about. As regards weight nothing more need be said here, because weight is a 'property' and properties will be discussed in the next chapter.

So far we have been considering perceptual objects of the kind that can be exemplified in many places at the same time, e.g. there are many events which can be known as 'white rabbit', but all perceptual objects are not of this kind. Consider, for example, the earth's satellite. There is only one moon. This is a perceptual object which is only exemplified in certain places at certain times as predicted by the Nautical Almanac. What is 'universal' (in the sense in which I have been using that term) about the moon is its mode of occurrence in space-time, since it is in virtue of this that we are able to recognize it as, on a given occasion, exemplifying the perceptual object 'moon'. In this example we have the following entities to consider: (1) the silvery disk and its spatial separation from the body of the percipient. As in the case of the ringing bell it is perceived as something going on at a remote but vague distance from the body. All this belongs to the primary realm. (2) The perceptual object of the plain man, which belongs to the realm of knowledge. (3) The moon of the astronomer—also belonging to the realm of knowledge but more extensive than the plain man's because the astronomer has penetrated further into that realm. This moon is 'spheroidal', has a 'far side' which is never perceived, etc. When I am perceiving (2) or thinking (3) I am knowing (1), an event, as (2) or (3) as the case may be. Thus percepts

or perceptual objects are concepts which are capable on a given occasion of direct exemplification in the primary realm. Without something sensed, i.e. without awareness of the primary realm there would be no concepts or percepts as far as natural science is concerned. But there can be concepts of something which cannot also be known as a percept. Some such concepts may later 'acquire' perceptual exemplification as was the case with the planet Neptune. Moreover a blind man can know the moon conceptually but not perceptually. The perceived spatial separation from the body of the percipient is vague in the case of the moon because there is no correlated tactual experience. But none the less, vague though it be, it is as much a datum of perceptual experience as the silvery disk is part of what is sensed.

Primarily we identify (2) the perceptual object with (1) the event which is *known* as it. Later when we learn a little elementary astronomy we add our conceptual moon to (2), and call it the physical moon. Then we learn a little physiology and perhaps come to wonder whether the silvery disk can have any relation to the physical moon. We may read phenomenalistic arguments and develop doubts about the existence and 'knowableness' of a 'physical moon'. We may then come to suppose that the conceptual moon (3) is a thought about the silvery disk or a thought about nothing! But it cannot be a thought about the silvery disk because many things are predicated about the event which is known as the moon which would never be predicated of the silvery disk. All these difficulties arise from the fact that we have abstracted the silvery disk from its spatio-temporal relations, and then made the mistake of supposing that our knowledge is confined to this bare abstractum.

It is because there is a systematic correlation between the silvery disk and space-time that knowledge of that which is known as the perceptual moon is possible. The astronomer relies on this systematic correlation when he points his telescope in the direction of the silvery disk. In this case it is the direction that matters—the direction in which he has to look in order to become aware of the silvery disk. This direction is correlated in a round about way with the spatio-temporal relations of the event which is known as the astronomer's body and the event which is known as the moon. The astronomer cannot measure the moon in the same way that

we measured the rabbit because tactual data are lacking. He therefore measures angles and clock-readings and much of the conceptual knowledge of the moon is reached in this way on the basis of the axioms of some four-dimensional geo-chronometry or other.

In this section I shall try to explain what appears to be the nature of the relation between thought-objects or concepts and that of which they are thoughts, so far as the concepts of natural science are concerned. Consider a map. This is a product of knowledge. It is as much an outcome of the intellectual activities as the artist's picture is an outcome of the aesthetic. But it is as it were 'embodied' knowledge, because the map also belongs to the primary realm. It can be known as a perceptual object. Now we make judgments of value about maps, just as we do about pictures. A *good* map is one which enables us to find our way about in the primary realm in which our bodies move. This, as we have seen, is a spatio-temporal realm, a realm of directions and distances, and a good map has to *correspond*, in accordance with a scheme of 'conventional signs' with those directions and distances. This is possible on account of the property of directions and distances in virtue of which they can be accurately represented on different scales. But by means of its conventions the map also tells us about the kind of perceptual objects which are likely to be exemplified on the way. Thus the map is representative of what it is knowledge of. By a good map we mean one which represents whatever it does represent *truly*. And this we test by seeing whether we do in fact find what the map leads us to expect. If a map were typical of conceptual knowledge in general (and all that can properly be called knowledge seems to be conceptual in the wide sense) then we should be able to say that knowledge is always representative (in the above sense) and our criterion of truth would be correspondence between the thought-object and whatever that object was a thought *of*. But is the case of the map typical? In the first place it is to be noted that it would be possible to have the knowledge which the map conveys without the map. We do of course have a great deal of topographical knowledge of the same type about regions with which we are well

acquainted without using maps. The actual map is a means of conveying topographical knowledge to others pictorially, i.e. through visual perception, and thus fulfils the same rôle as the written sentence or other visible symbol. Thus the map is doubly representative; it represents the actual directions and distances we have to traverse in getting from one place to another, on a reduced scale, and it is representative by 'embodiment' of our conceptual knowledge of such entities. Directions and distances happen to be the kind of entities which can be represented in this way. But there are plenty of thought-objects or concepts (for 'objects' do only belong to the realm of knowledge) which cannot be represented by such means as a map. If we run over our stock of scientific concepts we shall find numerous examples. Consider energy for example. We have a word for it and we think about it, but it cannot be represented pictorially and cannot be imagined. It can only be conceived or thought. Moreover it is not considered to be only an object of thought, but something else in another realm of the world of being is believed to exist to which our concept of energy refers.[1] But if we set about looking for perceptual exemplification of it in the hope of finding something of which we can say: This is energy, in the same way that we can say: This is a rabbit, we shall be disappointed. Thus we appear to discover entities which are not found to be immediately exemplified in direct perceptual experience, and such entities, not being known as objects of perception, cannot be represented in the imagination: they can only be thought.

Next consider a mathematical proposition: $3 + 2 = 4$. It is clear that the concept 2 is a pure object of thought not exemplified as such in perception. It is true that we can perceive entities which can be known as two things, but it is not the things but their twoness which we think with the concept 2. Moreover the notion of addition has a special meaning, because if we add two drops of water to two more drops we get not four but one drop, and even if we measure two ccs. of water and add them to two ccs. of alcohol we do not obtain four ccs. of the mixture. Thus there are concepts about which true propositions may be asserted—concepts of abstract logical relations—which are not supposed to have representatives in other realms in the same sense as that in which the concept

[1] Regarding energy see p. 117, below.

of energy is commonly supposed to be represented. Then there are such entities as electrons which are supposed to have representatives in the primary realm, but can they be represented pictorially when they are not supposed to be discoverable in perception? And if we do represent them pictorially how shall we tell whether the picture is true when we cannot test this in the same way as the pictorially representative map is tested? What tempts us to do so in this case is the fact that they can be imagined as little round particles and so forth. But we can no more test the imaginary picture than we could a painted one.

From these examples we see that the case of the map was not at all typical, but has many features not shared by all concepts. That is to say although, on the view here taken, all objects are concepts and some are perceptual objects (i.e. concepts having perceptual exemplification) by no means all concepts are objects. There does seem, however, to be one feature common to the cases examined—the fact, namely, that all concepts are representative *of*, i.e. thoughts *of*, something else. But this something may not be discoverable immediately through perception, and cannot therefore be represented pictorially. This distinction, as already mentioned in the Introduction, is of great importance on account of the way in which conceiving, perceiving and imagining are perpetually confused in scientific literature. It is another example of the way in which common-sense procedure is liable to mislead us. It happens that most of the objects of common-sense thought are perceptual objects and consequently our concept of, say, a chair can be represented by an image.[1] Hence common-sense thought largely consists of thinking by means of images—a mode of thinking which is limited to perceptual objects—either by revival of such in memory, or by free re-combination of 'parts' of perceptual objects. But as we have seen thought is much wider than perceptual thought. All our scientific thinking is done with concepts only some of which are objects and only still fewer of which are perceptual objects. Accordingly only a few can profitably be represented by images and we must be on our guard against the temptation to conceive such entities as atoms too naïvely after the model of perceptible balls. Rabbit, rodent, mammal are all names, i.e. symbols, for concepts, but they are not all

[1] See above, p. 78.

on the same footing as regards pictorial representation. As we pass from rabbit to mammal we become more and more abstract. 'This is a mammal' means: 'Here and now the primary realm is characterized by mammalness' and you cannot have an image of mammalness. If the primary realm is to be characterized by mammalness it must also be characterized by hairiness, milk-secretingness, etc., because this is what mammalness means. We think in meanings, not necessarily in images. It is characteristic of modern science that its concepts are becoming less and less picturable in the imagination and consequently more and more liable to be obstructed by the use of images and thus less and less amenable to common-sense ways of thinking.

Now it will be evident that for accurate thinking and fruitful discussion we must continue to mean the same, and our symbols must continue to have the same reference, i.e. there should, ideally, be one symbol for each concept. This almost trite precaution is, in spite of its obviousness, very frequently neglected by biologists. We perpetually find terms used in two or more senses without adequate definition, and in many cases it is evident that the author is not clear in his own mind what precisely he means by a word because he has neglected the precaution of trying to state it clearly. Some say this is not necessary, others that it is impossible. Thus Loeb wrote: 'Science is not the field of definitions but of prediction and control'.[1] But what sort of mathematics should we have if x meant one quantity on one side of an equation and another on the other? It will of course be answered that the subject-matter of biology does not admit of precise treatment, that there are no hard and fast lines in nature and so on. The last statement is profoundly true, but the answer is simply this: biology will be scientific precisely in so far as we do succeed in imposing 'hard and fast lines' on nature, in so far, that is to say, as we succeed in conceptualizing nature. The aim of biology is not pictorial representation or photographic reproduction of observations, but the creation of a systematized body of propositions about those entities which are known as living organisms, and clear concepts are required for this.[2] The following passage from Pro-

[1] *The Organism as a Whole*, London, 1916, p. vi.

[2] When I say that clear concepts are absolutely essential for scientific biology, I do not for a moment wish to suggest that vague hints and suggestions are not valuable. As Prof. Hobhouse says: 'The whole

fessor L. J. Henderson may be quoted in contrast to the above from Loeb:

> 'Life must by arbitrary process of logic be changed from the varying thing which it is into an independent variable or invariant, shorn of many of its most interesting qualities to be sure, but no longer inviting fallacy through our inability to perceive clearly the questions involved.'[1]

In this passage there is a recognition of the necessity for clearly defined concepts and of the fact that concepts are abstract—for to be abstract is precisely their function. We must follow the method of abstraction in biology if there is to be any science of organisms. But what is not clearly recognized in the above passage, nor in the well-known book as a whole from which it is taken, is the nature of the abstractive process. For one thing this is not, or should not be, merely 'arbitrary'. And in the second place although we thereby avoid the fallacies attendant upon lack of clarity we are liable to fall into others which are just as serious. These other fallacies depend upon the ease with which it is possible to forget what has been omitted in the abstracting process. The mistake is thus made of supposing that any method of abstraction can be exhaustive when from the very nature of the case this is impossible. Consequently the success of one method should not blind us to the desirability of exploring others.[2] Other fallacies spring from the failure to notice that even perceptual objects are abstract. What is not abstract—what is particular and concrete—is a given actual occasion of the exemplification of a perceptual object in the primary

growth of science has been a lesson in that form of intellectual humility which refuses to reject because it cannot understand. And at every step we have to believe in a something which we cannot, until we take the next step, define.' The point I have been urging is that such concepts cannot properly take their place in the body of a science until they have been defined. As the tender growing points of thought vague suggestions deserve every respect, and the sharp wind of criticism should not 'visit them too roughly'. But if they are admitted into a theory with a serious claim while still in the woolly state, they only bring confusion with them. Biology has been ruined in the past through the incautious use of woolly concepts.

[1] *The Fitness of the Environment*, New York, 1913, p. 36.

[2] On this important but persistently misunderstood point see especially: W. James, *Principles of Psychology*, II., 334-6; L. T. Hobhouse: *Theory of Knowledge*, pp. 197-8; A. N. Whitehead, *Science and the Modern World*, p. 23: 'the intolerant use of abstractions is the major vice of the intellect,' and p. 73: 'In so far as the excluded things are important in your experience, your modes of thought are not fitted to deal with them.'

realm. The perceptual object itself marks, as we have seen, the recognition of those aspects of the concrete occasion which are repeated or exemplified in other places or at other times. The process of abstraction is not arbitrary if the primary realm is not arbitrary. If there is systematic correlation in the primary realm then there will be principles governing the process of successful abstraction. These principles have only been discovered to a limited extent and in a few sciences and their elucidation has required the best efforts of the greatest geniuses.[1] Logic is not at all concerned with 'arbitrary processes'.

What has here been said about concepts can be illustrated by reference to the history of the concept of the cell in biology. The concept of the cell was the outcome of the *optical* analysis of organisms. It represented the picking out of one recurring *structural* feature common to the vast majority of organisms. First it meant the wall—the feature most obvious in plant cells, then the stuff within the wall, and finally the stuff was found always to be differentiated so as to possess an optically distinguishable body—the nucleus. The concepts 'cytoplasm' and 'nucleus' thus arose at the same time—the term cell being expressive of the way in which these things occurred in the animal and plant body. The concept was thus extremely abstract—all modifications and relation to the whole being left out of account. But in this form it could be generalized over all organisms, including Protista and the gametes of animals and plants. Thus all organisms were regarded as consisting either of a single cell, or of a mass of cells—the cell being regarded as the 'unit' of structure. With the advent of the doctrine of evolution the whole cell-theory was given a phylogenetic interpretation.

With the progress of investigation—with the discovery of chromidia, syncytia, etc.—changes began to take place in the meaning of the word cell. Also with the progress of research the old cell-theory came to be severely criticised, and with this has been confused criticism of the cell *concept*. Professor E. B. Wilson writes:

> 'Even to-day we cannot frame an adequate brief definition of the cell; but fortunately such a definition is unnecessary. In practice we need no more than the simple formula . . . a mass of

[1] Cf. the passage quoted from Dr. Broad above on p. 45; also the passage from Mill quoted as a 'motto' on the title-page.

> . This definition must not cease. Like most other definitions in natural science, it must be allowed a certain flexibility, but in respect to essential accuracy the old definition remains today unshaken by the advances of half a century.'[1]

Now compare with this the following opinion of another eminent cytologist, the late Professor Doncaster :

> 'As long as it was generally agreed that all organisms are built up of cells as a house is built of bricks, the description of the cell as a "unit" of living matter was not open to any very grave objection ; the cell was to the biologist almost what the atom was to the chemist . " . and the word had, in appearance at least, a fairly definite meaning. Now, however, when the old idea of discrete and independent cells is almost abandoned, and when distinguished biologists maintain that one whole group of organisms (the Protists) are non-cellular, the word "cell" is beginning to lose its definite and precise significance, and to be used rather as a convenient descriptive term than as denoting a fundamental concept of biology.'[2]

These two passages show very clearly the desirability of paying a little attention to *Erkenntniskritik* on behalf of biological thought, and particularly to this question of concepts. Let us see how far the view I have been expounding will help to clear up the apparent conflict and confusion in these two passages. In the first place, bearing in mind all that has been said regarding microscopical observation, it is clear that what the cytologist sees when he is said to be 'looking at a cell' is a perceptual object and therefore belongs to the world of knowledge and is in some degree abstract. It is what he knows something in the primary realm *as*. Now the concept 'cell' as defined by Professor Wilson is obviously highly abstract and that is why it is capable of being so widely generalized.[3] Much of the criticism of the cell-concept rests on a failure on both sides to understand the abstract character both of the perceptual object and the concept. In the first place the usual mistake (derived from common sense) is made of taking the visually perceived cell as a piece of 'concrete reality' to use a common expression. What is concrete is the event with its related events and 'sense-qualities', which is the total situation part of which is being discriminated and known as the visual perceptual cell. A still further and more

[1] *The Cell*, 3rd edit. New York, 1925, p. 21.
[2] *Cytology*, Cambridge, 1920, p. 1.
[3] 'His definition if not perfectly precise and definite, does denote "a real mental concept of biology." See below, p. 293 *et. seq.*

serious mistake may be made of 'reifying' the *concept* cell. When once these mistakes are made confusion is inevitable. The progress of investigation soon shows the inadequacy of these concepts to do justice to the subtlety of plant and animal organization, but the critics, having made the same mistake as their opponents, fail to see that their proper course is not to reject those concepts which, properly interpreted, are very valuable ones, but to seek for other modes of abstraction which will be complementary to them. Instead they point to the richness and complexity of the facts, and contrast this with the meagre emptiness of the concept—showing clearly that they expect the concept in some way to *exhaust* the facts, and therefore fail to understand that the value of the concept depends on its 'meagreness'. Their opponents, on the other hand, realizing the value of the concepts, but also failing to understand their abstract nature, cling to them as 'concrete facts' and are unable to appreciate the criticisms of their adversaries. The controversy between the supporters of the so-called 'organismal' and 'elemental' views of the organism rests largely on this failure to understand the nature of conceptual analysis, and to appreciate the fact that one mode of abstraction does not necessarily exclude all others. The training of the biologist is not favourable to the cultivation of the mode of thought requisite to the understanding of this aspect of such controversies, and accordingly these controversialists are apt to take sides in much the same spirit as candidates at a political election. 'All the onesidedness, the narrowness, and, above all, the intolerance of the world', writes Professor Hobhouse, 'comes from this inevitable abstraction of thought. And so the mind, though it must abstract, limit, ignore, is bound always to supplement its partial dealings; it must "strive always towards the whole", and if it cannot become the whole, it must try at least to understand its own limits'.[1]

Much more remains to be said about the cell concept and much remains to be done to clear away the difficulties raised by the two passages quoted above. But the discussion of specific biological problems will be undertaken in Part II and this topic will arise again there when we come to consider organization. Neither does it seem at all necessary to discuss here the ontological status of concepts because that belongs to

[1] *The Theory of Knowledge*, London, 1896, p. 7.

metaphysics. I think it is fairly safe to say that wherever it is possible to attach a definite meaning to a word that meaning is the concept. 'Each act of conception', writes William James, 'results from our attention singling out some one part of the mass of matter for thought which the world presents and holding fast to it, without confusion.' In another place he says: 'the mind can always intend, and know when it intends, to think the same'. But it need not be concluded from this that concepts cannot change, or better, that one cannot be abandoned and replaced by another. The important point is that one must know when such a replacement occurs, and make the necessary alteration in the definition of the word or symbol. That concepts do become obsolete and replaced by new ones is clearly seen in the history of the concept of the cell. Also I should add here that I do not believe the process of abstraction to be such a wholly arbitrary affair and one so entirely dependent upon 'subjective' factors as some pragmatists would have us suppose. That will be clear from what has already been said.

Before concluding this section it will not, I hope, be amiss to hark back for a moment to the historical point of view. At the time of Hume thinking was believed to be a process of passive contemplation of a panorama of discrete images with no intrinsic connexion with one another. The following passage suggests that Hume realized the inadequacy of this simple view:

> 'I believe every one who examines the situation of his mind in reasoning will agree with me that we do not annex distinct and complete ideas to every term we make use of, and that in talking of *government*, *church*, *negotiation*, *conquest*, we seldom spread out in our minds all the simple ideas of which these complex ones are composed. It is however observable, that notwithstanding this imperfection, we may avoid talking nonsense on these subjects, and may perceive any repugnance among the ideas as well as if we had a full comprehension of them.'[1]

It is clear that Hume is here referring to conceptual thinking, but with the psychological assumptions then current it was difficult to see how such a process was possible. He says that it is done 'by a kind of magical faculty in the soul, which, though it be always most perfect in the greatest geniuses . . . is, however, inexplicable by the utmost efforts of human understanding'. When Hume came upon difficulties of this

[1] *Treatise of Human Nature*, Bk. I., Part I., Sect. VII.

kind, instead of re-examining the assumptions upon which they depend, he either dismissed the difficulty as insoluble, or contented himself with a makeshift. For example, referring again to concepts in a later part of the *Treatise*, he says:

> 'All abstract ideas are really nothing but particular ones, considered in a certain light; but being annexed to general terms, they are able to represent a vast variety, and to comprehend objects, which, as they are alike in some particulars, are in others vastly wide of each other.'[1]

And yet, in spite of the fact that the above was written nearly two hundred years ago, in spite of the subsequent developments in epistemological theory and the improvements in our notions on these matters, the beliefs current among neurologists at the present day are hardly advanced beyond those of Locke. And the reason is not far to seek: instead of examining 'the situation of his mind in reasoning', which is the proper empirical way of proceeding, the neurologist prefers to speculate about what he supposes to be going on in his head, and to 'deduce' the truth about knowledge from this, and so comes to conclusions which are not only plainly contradicted by the use of the empirical method but would make all knowledge of heads and their contents impossible. This I take to be but another instance of the 'blinding influence' of current metaphysical notions in natural science and of the way in which these raise a barrier between the natural and the philosophical sciences, rendering any *rapprochement* between them impossible.

Another very common mistake is to speak of the truth or falsehood of concepts. But concepts can no more be true or false than colours or smells. Such predicates as true and false can only be applied to propositions. The *concept* 'centaur' is not false, but if I say: Centaurs are kept at the Zoo, this *proposition* may very well be false. Similarly: Centaurs are not kept at the Zoo, is a true proposition. But Karl Pearson says that centaur is a 'self-negating idea' and a 'compound of sense-impressions, which are irreconcilable anatomically'. Now in the first place 'sense-impressions' (by which is meant sensa) are not anatomical entities, and in the second place concepts are not compounded of sensa. The latter belong to the primary realm the former to the realm of knowledge. That there are entities in nature which can be

[1] *Op. cit.*, Bk. I., Part II., Sect. III.

'known as' centaurs is not in the strict sense of the term *impossible*, but on our present biological knowledge very highly *improbable*. It is no more nor less impossible or 'irreconcilable anatomically' than the surgical operation depicted by Karl Pearson himself which at some future date will link up two people's brains so that they can experience each other's 'sense-impressions'. Karl Pearson also says that 'In order that a conception may have scientific validity it must be self-consistent, and deducible from the perceptions of the normal being'. But what can be meant by saying that a concept is self-consistent? I can understand that two propositions may be consistent because each asserts something, but a concept asserts nothing and is neither true, false, nor consistent. Moreover concepts are not 'deduced' from perceptions. What Karl Pearson seems to be trying to say is that some concepts are valuable in science and others are not. We do make judgments of value about concepts depending largely on their success in generalization. The concept centaur is of no *positive* value in biology but it is not entirely devoid of significance as is shown by the fact that the proposition 'Centaurs are not kept at the Zoo' is perfectly comprehensible and true. All that need be said about it is that it does not happen to be very interesting. Even so it need not be concluded that untrue and negative propositions are entirely devoid of importance. Thus Professor Whitehead, speaking of what he calls the 'realm of alternative inter-connected entities', says:

> 'This realm is disclosed by all the untrue propositions which can be predicated significantly of that occasion. It is the realm of alternative suggestions, whose foothold in actuality transcends each actual occasion. The real relevance of untrue propositions for each actual occasion is disclosed by art, romance, and by criticism in reference to ideals. It is the foundation of the metaphysical position which I am maintaining that the understanding of actuality requires a reference to ideality. The two realms are intrinsically inherent in the total metaphysical situation. . . . To be abstract is to transcend particular concrete occasions of actual happening. But to transcend an actual occasion does not mean being disconnected from it.'[1]

[1] See the chapter on 'Abstraction' in *Science and the Modern World*. By an occasion Whitehead here means an 'occasion of cognition' an 'actual occasion of experience.' For other recent discussions on these topics see: L. S. Stebbing, *Abstraction and Science*, Journ. of Philosophical Studies, Vol. II., No. 5, p. 28; and N. Kemp Smith, *The Fruitfulness of the Abstract*, Proc. Aristotelian Soc. Session 1917-8, p. 203.

We now have to turn to the confusion between conceiving and imagining to which reference was made in the last section. We often find scientific writers condemning this or that theory as 'inconceivable', when what they frequently mean is unimaginable. Mechanists say that vitalism is inconceivable, and vitalists say that mechanism is inconceivable. What is called the 'transmission of acquired characters' is declared to be inconceivable, and, it is sometimes added, 'therefore impossible'. All that is meant is that no 'mechanism' can be *imagined* which would 'account for' it. I do hope that no zealous heresy hunter will draw from this the startling 'conclusion' that I am surreptitiously advocating the doctrine of transmission of acquired characters, and that no supporter of that doctrine will seize upon these innocent remarks in the belief that they favour his contentions. Such possibilities may seem 'inconceivable' but they have happened before and to guard against the twin demons of misunderstanding and misrepresentation no precaution can be neglected. All I am concerned with at present is to point out that imagination has really nothing to do with such questions, and that thought, and still less the possibilities of nature, are not limited by our imaginations. A remark of William James is very much to the point here: 'How do we know *which* things we cannot imagine unless by first conceiving them, meaning *them* and not other things?' Properly speaking inconceivable means *unmeaning*. To assert that a theory is inconceivable is to assert that it is meaningless. To assert that it cannot be imagined may be quite true, but it is not an assertion of much importance. It is deplorable that such a confusion should so often be made when it has so frequently been pointed out by the older writers. Thus Descartes, in Part IV of the Discourse on Method, speaking of his investigations of geometrical demonstrations, says that in the first place he observed:

> 'that the great certitude which by common consent is accorded to these demonstrations, is founded solely upon this, that they are clearly conceived in accordance with the rules I have already laid down.'

In the second place he perceived that:

> 'there was nothing at all in these demonstrations which could assure me of the existence of their object: thus, for example,

> supposing a triangle to be given, I distinctly perceived that its three angles were necessarily equal to two right angles, but I did not on that account perceive anything which could assure me that any triangle existed.'

Later on he writes:

> 'But the reason which leads many to persuade themselves that there is a difficulty in knowing this truth, and even also in knowing what their mind really is, is that they never raise their thoughts above sensible objects, and are so accustomed to consider nothing except by way of imagination, which is a mode of thinking limited to material objects, that all that is not imaginable seems to them not intelligible.'

The following passage from the 'occasional thoughts' of Boyle is worthy of quotation in illustration of the same point:

> 'When I say that spirit is incorporeal substance . . . if he should answer, that when he hears the words incorporeal substance, he imagines some aerial or other very thin subtil and transparent body, I shall reply, that this comes from a vicious custom he has brought himself to, of imagining something whenever he will conceive anything, though of a nature incapable of being truly represented by any image in the fancy. . . . Because the use of imagining, whenever we would conceive things, is so stubborn impediment to the free actings of the mind, in cases that require pure intellection, it will be very useful, if not necessary, to accustom ourselves not to be startled or frighted, with everything, that exceeds or confounds the imagination, but by degrees to train up the mind (if I may so speak) to consider notions that surpass the imagination, and yet are demonstrable by reason.'[1]

A particularly good illustration of the importance of these remarks of the 'father of modern chemistry' in a sphere in which they are specially apposite is furnished by a well-known saying of C. Mercier's in an argument against what is called psycho-physical interaction. He wrote: 'Try to imagine the idea of a beef-steak binding the molecules (of the brain) together. It is impossible.'[2] But it is equally impossible to 'imagine' any remark more utterly irrelevant. The first and most obvious point in this example is that what we can or cannot imagine in such a situation is of no significance whatever. We cannot imagine anything binding molecules together because molecules are not known in the manner of railway waggons. They are concepts—what events are known as. Neither can we imagine anything

[1] Robert Boyle, *Opera*, London, 1772, VI., 688.
[2] *The Nervous System and the Mind*, London, 1888, p. 9.

binding such events together, although we may conceive them as 'bound together' by other events. The 'idea of a beef steak' is a concept capable of perceptual exemplification as a perceptual object. It also is what certain events may be known as, but what its connexion with the brain or with that which is known as the brain may be, assuming it has any, nobody knows. The *process* of conceiving a beef steak may have some connexion with the event which is known as the brain, but what the nature of that connexion may be again nobody knows. But the main point is that nothing that the fancy happens to picture to itself about such entities is any guide whatever to what goes on in the brain, still less does it provide a safe basis for dogmatizing about those events. This may be a pity but 'tis true, and it is useless to go on kicking against the pricks of sheer hard facts. Our best course is to follow Robert Boyle's advice—not to be frighted with every thing which confounds the imagination but to train up the mind to consider notions that surpass it.

In concluding this chapter I will try to summarize its main points, particularly those which have been offered as an 'alternative to Phenomenalism'. The central contention has been the necessity for revising the notion of 'object'. I have attempted to discriminate a number of empirical 'realms' in our experience, but only so far as natural science is concerned. I have assumed that there are entities of some sort called percipients which indulge in other activities besides perceiving. These form a realm of their own, but I have said nothing about this because its investigation belongs to psychology, not to natural science in the sense defined in the Introduction, Section 6.[1] Then I have distinguished the 'Primary Realm' which is the realm we become aware of in the course of conscious life through sense-perception in so far as it is merely 'given' and not interpreted. In it we are able to discriminate spatial volumes during periods of time to which are related the sense-qualities, including bodily aches and pains. I do not suggest that all this is given in a single act of infantile perception, but I do suggest that it is given in the course of the transactions of the bodily events, on the one

[1] See above, p. 31.

hand, to which we as percipients suppose ourselves to be related, with the other events in the primary realm, on the other. It will be evident now that this primary realm, or part of it, is what is meant by 'nature' as defined in the Introduction, and henceforth the more familiar term will be employed. I make the assumption that the constituents of this nature are in some way organized or systematically correlated with one another. Karl Pearson prefers to assume that it is a chaos, but in that case the nervous system will also presumably be a chaos, and in that case it is difficult to understand how, if the nervous system has the importance which Karl Pearson attaches to it, any knowledge of nature will be possible. I take it to be the business of natural science to investigate the systematic correlation of the constituents of nature.

Finally I distinguished the realm of knowledge, the principal constituents of which appear to be concepts and propositions. These also are systematically correlated and the investigation of this correlation as far as their formal relations are concerned is the business of logic. It is here that I would place 'objects' because if we suppose them to belong to nature we expose ourselves to all the sceptical arguments of subjectivism based on the 'conflicting testimony' of the senses regarding the 'same' object.[1] There is no reason why a spatio-temporal region of nature should not have different sensa related to it from different standpoints or under different circumstances. But if we suppose that there are objects in nature which 'have' these sensa, then we are driven into inextricable confusion and contradictions. Objects, then, belong to the realm of knowledge, and are what chunks of nature are 'known as'. And the knowableness of nature in this way depends upon the systematic correlation of the constituents of nature in virtue of which there is sameness or 'repeatableness' amid the perpetual flux of events, so that many *different* events may be known as the *same* object. Professor Whitehead places perceptual objects as well as sensa in nature. I agree that sensa belong to nature and that they are different from events, but I do not call them objects, in order to avoid confusion with perceptual objects which do not appear to belong to nature. I am not sure that I understand Professor Whitehead's

[1] 'But the permanences in virtue of which nature is knowable by means of objects' belong to nature.'

' control theory ' of perceptual objects,[1] and his whole theory of objects does not seem to be so satisfactory as his theory of events. Nevertheless his contributions seem to be among the most original and profound that have ever been given to the science of epistemology, and it is therefore with all diffidence that I adhere to the view that in all cases when we speak of objects, whether perceptual objects of a relatively low degree of abstraction, or such objects as atoms or electrons where a high degree of abstraction is involved, we have to do with entities of thought in terms of which nature is known. Professor Whitehead himself does not teach that objects are ' in ' nature in any simple sense, and he protests against ' simple location '.[2] They are only derivatively spatial, and sensa stand in complex multiple relations to events. Perceptual objects on the other hand have a simpler relation to events, and for me this is expressive of enduring features of the organization of nature in virtue of which some events are such that the relation of certain sensa to them is more permanent than others, and they can accordingly be known as the familiar perceptual objects of daily life, or the more precise because more abstract purely conceptual objects of physical science.

As regards the mutual relations of these ' empirical realms ' and their relations to any other realms there may be, I have tried to abstain from saying anything about these because I take that to be the business of metaphysics, and I am anxious to differentiate as far as possible the respective tasks of natural science, epistemology and metaphysics. But it will now be seen that it is impossible even from the standpoint of epistemology to avoid making *some* assumptions of this kind. I have assumed that percipients have direct experience of sensa, that they are capable of apprehending spatio-temporal relations in the course of conscious experience and of reaching concepts by means of which they are said to ' know ' nature. But I have not attempted to ' explain ' knowing because this

[1] See *Uniformity and Contingency*, Proc. Arist. Soc. XXIII., 1923, p. 5 *et seq.*, and *Principle of Relativity*, Chaps. II. and IV. Also p. 181 below. See also L. S. Stebbing, *Prof. Whitehead's ' Perceptual Objects '* Journ. of Philosophy, Vol. XXIII., 1926, p. 197.

[2] It is perhaps desirable to point out in passing that Prof. Whitehead's theory of knowledge is most emphatically *not* a ' form of naïve realism,' although realism it certainly is and ' rooted in naïve experience ' as a good epistemology should be. See L. J. Henderson in *Quart. Review of Biology*, Vol. I., p. 292.

does not seem to be part of the business of epistemology.[1] The fact that you do not know *how* you know something is no ground for denying that you *do* know it. The cognitive relation seems to be a relation *sui generis* and attempts to compare it with other relations do not seem to have met with much success.

If any reader finds this chapter too sketchy, or that obvious difficulties have been treated too cursorily, I shall entirely agree with him. But I would remind him that if we delay too long over epistemology we shall never come to biology. Neither of these sciences can claim to be complete. The further justification of what is here set down will, I hope, be found in its applications in Part II. In the next chapter we shall consider some of the 'general ideas which are indispensably relevant to the analysis of everything that happens', but with a view to the special needs of biology.

[1] Cf. the following from Kant *Prolegomena*, § 21a: "It is first necessary to remind the reader that it is not here a question of the origin of experience, but of its contents. The first belongs to empirical psychology, and would itself never be capable of proper development without the second, which belongs to the critique of knowledge and especially of the understanding."

CHAPTER III

The Categories of Substance and Causation and their Use in Modern Natural Science

When the word 'substance' is used by a biologist it is usually preceded by the article 'a' and the adjective 'chemical'—he speaks, for example, of sodium chloride as a chemical substance, and of 'protoplasm' as a 'mixture' of chemical substances. And a particular crystal of sodium chloride he thinks of as a specimen or example of the substance 'sodium chloride'. This use of the word substance is quite different from its use in philosophy, or at least from some of its uses. But nevertheless we use the category of substance—i.e. the notion or notions to which philosophers apply the term—in our scientific and common-sense thinking, and therefore it requires careful consideration. We shall first examine its philosophical meanings, and then study its more or less subterranean workings in biological thought. The category of causation will be found to be closely related to that of substance, and I shall try to show that both categories are manifestations of one fundamental mode of thought—namely the search for permanences in nature. This is true, of course, generally of concepts, as was said in the last chapter, but the category of substance has often been applied to what was regarded as absolutely permanent.

It is desirable to look first at the definitions of substance given by some of the great thinkers, in order to understand its philosophical meanings. Thus Aristotle wrote:

> "Substance, in the truest and primary and most definite sense of the word, is that which is neither predicable of a subject nor present in a subject; for instance, the individual man or horse."[1]

He explains that by being 'present in a subject' he does not mean 'present as parts are present in a whole' but 'being

[1] *Categories*, Eng. trans. Oxford, 1926, Ch. 5.

incapable of existence apart from the said subject'. Further on he also says that 'primary substances are most properly called substances in virtue of the fact that they are the entities which underlie everything else, and that everything else is either predicated of them or present in them'. Now in continuation of the first passage quoted Aristotle introduces another sense of the term substance:

> 'But in a secondary sense those things are called substances within which, as species, the primary substances are included; also those which, as genera, include species. For instance, the individual man is included in the species "man", and the genus to which the species belongs is "animal"; these, therefore—that is to say, the species "man" and the genus "animal"—are termed secondary substances.'

This will do for the present for Aristotle's use of the term.

Descartes understood by substance 'a thing which exists in such a way as to stand in need of nothing beyond itself in order to its existence'. He adds: 'And in truth, there can be conceived but one substance which is absolutely independent, and that is God.' But he extended the term to cover two subordinate substances, in the following way:

> 'Created substances, however, whether corporeal or thinking, may be conceived, under this common concept; for these are things which, in order to their existence, stand in need of nothing but the concourse of God. But yet substance cannot be first discovered merely from its being a thing which exists independently, for existence by itself is not observed by us. We easily, however, discover substance itself from any attribute of it, by this common notion, that of nothing there can be no attributes, properties, or qualities: for from perceiving that some attribute is present, we infer that some existing thing or substance to which it may be attributed is also of necessity present.'[1]

This passage is interesting in connexion with what was said about Aristotle and the Renaissance in the Introduction (p. 42) showing as it does how some Aristotelian notions regarding logic and metaphysics survived the other changes at that time.

Spinoza's definition of substance is as follows:

> 'I understand Substance to be that which is in itself and is conceived through itself: I mean that, the conception of which does not depend on the conception of another thing from which it must be formed.'[2]

These three quotations reveal most of the ideas to which the category of substance gives expression: (1) the individual

[1] *Principles of Philosophy*, LII. [2] *Ethics*, Part I., Def. III.

thing. (2) the natural kind (species or genus) of which the individual thing is an example—Aristotle's secondary meaning of the term substance; (3) the permanent in reality which is not dependent upon anything else; (4) that to which the dependent entities are related as attributes, qualities or properties.

When we come to the empiricist writers all this seems to be changed. The psychological assumptions upon which they founded their view of knowledge—particularly their way of treating sensa as original, elementary and isolated—inevitably lead to scepticism regarding the notions of substance and cause. Locke ridiculed the notion of an entity 'not observed by us' which is required to 'support' the attributes. Yet he himself believed in what he called the 'corpuscularian hypothesis' of matter as consisting of imperceptible particles with 'powers to produce' ideas in us 'by impulse; the only way we can conceive bodies to operate in'—and this is nothing but a particular example of the use of the categories of substance and causation. But regarding our 'idea of substance in general' Locke wrote:

> 'So that if any one will examine himself concerning his notion of pure substance in general, he will find he has no other idea of it at all, but only a supposition of he knows not what support of such qualities which are capable of producing simple ideas in us, which qualities are commonly called "accidents." . . . The idea, then, we have, to which we give the general name "substance," being nothing but the supposed but unknown, support of those qualities we find existing, which we imagine cannot subsist sine re substante, "without something to support them," we call that support *substantia*; which, according to the true import of the word, is, in plain English, "standing under," or "upholding."'[1]

Locke is vague about how we come by the notion of matter but in the fourth book where he discusses knowledge in general, he distinguishes three kinds of knowledge: intuitive, demonstrative, and what he calls 'sensitive'. It is by means of the last that we have assurance of the existence of entities other than ideas—'with an evidence that puts us past doubting':

> 'for I ask any one, whether he be not invincibly conscious to himself of a different perception when he looks on the sun by day, and thinks on it by night; when he actually tastes wormwood or smells a rose, or only thinks on that savour or odour?'[2]

[1] *Essay concerning Human Understanding*, Bk. II., ch. XXIII., 2.
[2] *Ibid.*, Bk. IV., ch. II., 14.

But Hume took it for granted that 'the difference betwixt these consists in the degrees of force and liveliness, with which they strike upon the mind, and make their way into our thought or consciousness'. When he comes to examine substance he follows his usual custom of seeking for the 'impression' to which an 'idea' could be traced, and not being able to find one, concluded that we have 'no idea of substance, distinct from that of a collection of particular qualities, nor have we any other meaning when we either talk or reason concerning it'. He adds that the qualities are 'commonly referred to an unknown *something*, in which they are supposed to inhere; or granting this fiction should not take place, are at least supposed to be closely and inseparably connected by the relations of contiguity and causation'.[1]

It is interesting to contrast these classical opinions about substance with the following passage from a modern scientific writer which has already been quoted in the Introduction and, for convenience, is repeated here:

> 'Energy is the underlying cause of all changes in matter. This does not seem a very satisfactory definition but, so far, it is the only one possible. It is a very striking fact that the two fundamentals of our external world, matter and energy, have, for us no existence apart from their effect on us. We cannot prove that there are such things except in so far as they manifest themselves, matter by being changed, and energy by producing changes, which in turn alter our sensation-complex.'[2]

If any one believes that modern science is not 'metaphysical' let him consider carefully the above passage in the light of our foregoing discussions. If he needs more data he can also consult the annual presidential addresses at the British Association for the Advancement of Science. It seems clear from the above that in physics (as understood by the author of this passage) the category of substance is exemplified by matter and energy. But like the substances of Descartes they are 'not observed by us'. Alterations in our 'sensation-complex' are, it seems, first interpreted as resulting from changes in matter. But this is not deemed sufficient and energy has to be invoked to *produce* the latter changes. Thus energy seems to be at one remove more from 'knowableness' and is as 'metaphysical' and remote as the scholastic *substantia* which Locke ridiculed and Berkeley and Hume demolished.

[1] *Treatise of Human Nature*, Bk. I., Pt. I., sect. VI.
[2] See above, p. 46.

But of course it is not true that the above 'definition' of energy is 'the only one possible'. There is another one which defines it as the product of half the mass of a body into the square of its velocity, and *this* is the definition actually used in physical science, as contrasted with the metaphysics of science. We shall return to energy again in the next chapter.

Hume's celebrated criticism of the notion of causation rested, like his criticism of substance, on the failure to find the corresponding impression. He was unable to find it in any quality of objects, and among relations contiguity and succession did *not* suffice because 'An object may be contiguous and prior to another, without being considered its cause.' The notion of necessary connexion was rejected on account of two principles which, he says, 'I cannot render consistent nor is it in my power to renounce either of them.' These were:

> 'That all our distinct perceptions are distinct existences, and that the mind never perceives any real connexion among distinct existences.'

His general conclusion was that we never perceive a causal nexus between things, and that our inductive inferences are based upon custom or habit. Two sentences in Section XII of Part III of the first book of the *Treatise* summarize his conclusions:

> 'that there is nothing in any object, considered in itself, which can afford us a reason for drawing a conclusion beyond it.'
> 'that even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience.'

Substance and causation were rehabilitated by Kant in a peculiar form. Far from finding that we had no such ideas Kant held that thought would be impossible without them. But, agreeing with Hume that they were not to be found in experience, he concluded that they were a priori in the sense that we are equipped prior to experience with these ways of thinking in, to put it crudely, a blank form, ready to be employed upon the materials furnished by sense.

We can now leave these historical opinions and turn to study the working of these categories in scientific and common-sense

thought. It is important to bear in mind the common-sense usage because if we are not on our guard this is very liable to influence our scientific thinking unduly.

Permanence and change are two pervasive elements in our experience. The category of substance gives expression to the unchanging, and causation is wrapped up with the notion of change; not, however, with change as such, but with what we may call 'repeatable' changes—changes which 'crop up' again and can consequently be *recognized* as changes of a particular kind. The category of substance is exemplified, in one of its meanings, in the common-sense thing, and the prototype of the thing is the solid. We are always most at home with solids—they are clear cut and, as we say, 'tangible', and common-sense thought is lost in the absence of the 'tangible'. Gases were not properly studied till the second half of the eighteenth century, and both gases and liquids are treated in scientific thought in terms of solids, i.e. as consisting of solid particles. Similarly changes wrought by pulling and pushing have yielded the type of the causal process. Consequently the solid body, force, and motion have been the three fundamental concepts in the history of physics. Connected with this is the notion that an inanimate thing will not change unless something else *makes* it change. If, therefore, we find change going on we are led to look for some antecedent change in another thing upon which we can place the responsibility for this change.

This search for the permanent—which has dominated scientific thought no less than philosophical—culminates in natural science in the doctrines of the conservation of matter and energy. But let us look first at some less abstract examples of permanences in nature—less further removed from what is perceived. We will examine the following instances: (1) a stone or crystal; (2) a persistent animal type, e.g. Brachyopod genera; (3) an inert atmospheric gas, e.g. argon; on a *prima facie* view these might be described as permanences of things. (4) The fall of a stone; (5) the living mammalian heart; (6) the disintegrating radio-active 'chemical substances' or 'stuffs' as we may call them in order to avoid confusion with the other uses of the term substance. These three instances may be distinguished provisionally as permanences of process. (7) A quantitative permanence—such as persistence of weight in spite of chemical change, we can

call a permanence of property. We must now compare these examples in detail.

(1) A stone or a crystal conforms very well to the common-sense ideal of an enduring Thing. It may remain with little or no change—perhaps for years. But the stone and the crystal differ in an important respect. The form of the stone, if it is of an amorphous material, results from the contingent circumstances of, say, its history in the sea or river bed. But the form of the crystal appears to be something essential to it (although of course not independent entirely of contingent circumstances) and might be described as an intrinsic character. Thus, in a sense which will become clearer as we proceed, the crystal has more 'individuality' or 'thinghood' than the stone. But the crystal, like the stone, is liable to be at the mercy of a casual blow with a hammer, when it will cease to exist as a single common-sense thing, although its substance still persists as a definite chemical stuff. This brings us to type (3). Being a gas this example lacks spatial boundary and tangibility, and so does not conform to the requirements of a common-sense thing. It has a low degree of thinghood, but it is a good example of an enduring or 'stable' chemical stuff. Type (2) again, but for a different reason, would not be called a thing, but it is an example of a 'substance' in Aristotle's 'secondary meaning' of the term (cf. p. 171), and a very permanent one too. Some plant and animal types appear to have endured with astonishingly little change from the most remote epochs, as though the whole march of evolution had passed over them and left them unheeded. A chemical stuff, e.g. copper sulphate, is also an example of an Aristotelian substance in the 'secondary sense', which differs from the animal type in an important respect, namely in the nature of the relation between the individuals constituting it.

We turn now to 'permanences of process'.[1] beginning with type (4)—the fall of a stone. By calling this permanent all I mean is that however many times we care to pick up and release it the stone will fall. We should be very much astonished (although if we are disciples of Hume we ought not to be) if the stone did not fall, and we should at once set about looking for something—some other thing—which *prevented* it from falling. Thus there seems to be something permanent

[1] By this I do not mean processes which do not cease but processes which *recur* from time to time and belong to the same *type*.

about the mutual relations between the stone and the earth which is expressed in this repeated falling. It is a typical case of a causal relation between two things. It used to be assimilated to the familiar pulling type by supposing that the earth exerted a pull upon the stone through an impalpable medium—another substance. Thus what is permanent about this example is the mode of happening—*that* it will fall if released, also the *rate* of fall. But there is also something contingent too, namely, *when* it is released, or *what* releases it, since this depends upon some third thing.

Type (5), the beating heart, is another example of permanence of process which presents many important differences from the first. In the first place, it is not a process occurring between two things, but something happening in one thing. Moreover this one thing has only a small degree of thinghood from the point of view of independence, because it is an organic *part*—a part of an organism possessing a higher degree of thinghood than itself. It is not a substance in the primary sense of Aristotle because, 'in nature' at least, it is 'incapable of existence apart from the said subject'[1] i.e. the whole organism. In the second place, it differs from the first example in being less contingent. Being a rhythmical process *when* the beat happens is related by a definite (although not invariable) temporal interval to earlier and later beats. Moreover, although the rate is dependent upon other events in the body, it begins beating before the nerves reach it, and the whole heart and even small fragments will go on beating for longer or shorter periods according to circumstances, after they have been removed from the body. Thus the *rhythmical character* of this process seems to be in some way an *intrinsic* one since it is largely independent of other processes external to the organ, and is almost certainly not dependent on external rhythmical processes.

Example (6) the disintegrative process of a radio-active stuff is peculiar in being serial and not rhythmical. It also differs from type (4) in being to a very high degree independent of contingent circumstances. It is not explicable by reference to other serial processes going on elsewhere in direct causal relation to it. In this intrinsicness of character it resembles the beating heart, but differs from the latter not only in not

[1] See above, p. 171.

being rhythmical but also in being associated with a kind of chemical stuff, not with an organic part.

We now come to type (7)—permanence of property. When a piece of zinc is dissolved in a mineral acid the weight of the whole at the end of the process (provided we take care to include the gases that are evolved) is the same as that of the sum of the separate constituents—or such differences as may exist are regarded as due to errors of technique—in spite of the fact that neither zinc nor acid are any longer discoverable. Thus although zinc and acid as such disappear one property remains constant and persists. What we observe in this case is a permanence of property. This notion of property requires further analysis.

When a thing e.g. a crystal, continues to manifest the same qualitative characterization without change we speak of it as existing in a state. If the crystal is placed in water and dissolves we speak of it as exhibiting a property. The term property commonly signifies a process of a certain recognisable type which is exhibited by a thing when it is placed in a certain definite relation with certain other things. The manifestation of a property involves, in many cases, the disappearance of the thing as such. Consider, for example, the case of the property called solubility in water. We cannot say that a certain crystal 'has' the property of dissolving in water by mere perceptual looking or touching, nor can we say so on the grounds that we have tried it and found it to be so, because a crystal ceases to exist when it is dissolved in water. We can say it possesses this property only if we know that it belongs to a particular kind of stuff which is known to be characterized in this way. We then argue deductively that this particular crystal will also dissolve in water. Thus although predicating a quality of a thing involves reference to a percipient, this does not appear to be the case with predicating properties. The exhibition of a quality does not involve change in the thing, whereas the exhibition of a property does involve change, and a property can only be discovered if change takes place. Thus when, ordinarily, we predicate a certain property of a thing this involves: (1) that it is known to belong to a certain class of things, and (2) that

when in certain relations with certain other things it, or the other things, will manifest a recognizable type of change. Thus a thing is not only a group of sense-qualities organized in a recognizable way, but is also thought of as the seat of certain 'permanent possibilities' of types of change. And there are two fundamental ways of classifying things in natural science: one employed in the physical sciences, and one employed in biology. The method adopted in the first case is in terms of kinds of stuff which are recognized by means of properties of the kind called chemical. Ice, water and steam manifest very different qualities and properties, but they are not regarded as belonging to different kinds of stuff, and the change from one to another is not regarded as a chemical change. But if the steam is passed over red-hot iron filings what issues from the tube is not regarded as being of the same kind of stuff, and what is left behind is no longer regarded as belonging to the kind called iron. Changes of this kind are called chemical because they are interpreted by means of the notion of composition, i.e. in terms of imperceptible entities which undergo changes of arrangement. The same is true of the change from water to steam, but in this case only particles of the water itself are regarded as involved, whereas in the second case not only are particles of the iron involved but a further splitting up of the water-particles is required in the explanation. And this is accordingly regarded as a change of a much more profound type.

In all the seven examples of 'permanences in nature, examined in Section 2 there is one common feature: each exhibits some feature which is persistent, pervasive or regular —some feature which 'turns up again' and can therefore be recognised and so offers a foot-hold for knowledge. We also notice that in each case there is something contingent too—connected chiefly with the dependence of one thing upon another. Since properties always appear (in natural science) to involve process we may say that our types fall under two heads: (1) permanences of Things, and (2) permanences of Process. But this is not satisfactory because some of the permanences of process were processes characterising single things (e.g. the beating heart). Moreover some of the 'things'

did not possess the marks of common-sense 'thinghood', and some of the processes always involved two or more things and cannot be observed in single things. We require a further analysis of the notions of things and thinghood and of types of permanence of process. We shall then be better prepared to examine causation.

To this end we must recall the results of the epistemological discussions of the preceding chapter. We saw that a perceptual situation involves: (1) awareness that 'something is going on' with a space-like separation from our bodies; (2) awareness of sensa related to the something going on; (3) knowing this something as, or *in terms of* (4) something else—usually a solid perceptual object, which is analysable in thought into the sensa under (2) if we neglect the spatio-temporal relations involved. But we have now seen that in knowing something as, say a crystal of copper sulphate, I am not only knowing it as a certain blue crystalline form, but also as 'soluble in water'. But this is not a character of the perceptual object, but a character of the something which is known perceptually as the perceptual object. Common-sense thought, however, regards the property as a predicate of the object, and in natural science we tend to identify the permanences of process we discover with *things*, either perceived or hypothetically postulated for the purpose, e.g. Mendelian genes are conceived as material particles. Even energy and its various 'forms' are apt to be conceived as stuff—as when people speak of it as passing from one thing to another, or as being 'stored up' in the carbohydrate molecule, ready to be 'released' in the metabolic processes of the organism

We have already used the Whiteheadian term 'event' for the 'something going on'. Then a perceptual object—what an event is known as—marks the recognition of a character of an event. And we also see now that a 'property' is also a character of an event, or of a complex of events, although we usually think of it as a property of the object in terms of which we know the event. An event which can be known as a common-sense thing is one which has a certain degree of stability and permanence of qualitative characterization. Thus our stone is an event since it is temporally extended, but any section of its time-dimension during a long period will manifest much the same characterization as any other, i.e. the stone can still be recognized or known as a stone. Thus

one type of persistence—persistence of one thing in a state—is persistence of a mode of characterization of an event. Thus events are substances, in one sense of the term 'substance' as that to which other entities, e.g. sensa, are related as attributes or 'adjectives', but not in the simple two-termed relation of substance and attribute as in Aristotle's logic. On the other hand, a perceptual object or a property is expressive of a more or less enduring character attaching to an event in itself, and it is this fact which is taken into consideration in the 'control theory' of perceptual objects as was mentioned in the last chapter (p. 168). At this point it will be well to look at some of Professor Whitehead's sayings about events.

> 'What we d
> period of time.
> some specific c
>
> "I give the name "event" to a spatio-temporal happening. An event does not in any way imply rapid change; the endurance of a block of marble is an event. . . . By this I do not mean a bare portion of space-time. Such a concept is a further abstraction. I mean a part of the becomingness of nature, coloured with all the hues of its content."[1]
>
> 'An event is qualified space-time—or rather, the qualities and the space-time are both further abstractions from the more concrete event.'[2]

In his earlier book—*The Principles of Natural Knowledge*[3]—Professor Whitehead uses the word event in a more abstract sense to indicate 'slabs' of space-time. He speaks of natural elements and their relations. Homogeneous relations are relations relating elements of the same type. 'Extension' is a fundamental homogeneous relation and events are the relata thereby related and from which space and time are abstractions.

Now this is a complete inversion of the usual way of regarding nature. On the traditional view space and time are two unconnected kinds of relations between material objects. But modern physics assimilates space and time and, on the view of Professor Whitehead here advocated, material objects are characters of space-time.

[1] *The Concept of Nature*, Cambridge, 1926, p. 52 [1st edit. 1920].
[2] *Principle of Relativity*, Cambridge, 1922, p. 21.
[3] *Uniformity and Contingency*, Proc. Arist. Soc. XXIII., p. 14.
[4] Cambridge, first ed. 1919, second, 1925.

> 'It is difficult to understand how time can be a relation between two permanent objects. Also with the modern assimilation of time and space, this difficulty in respect to time also attaches to space. Furthermore, I hold that these permanent objects are nothing else than adjectives of events.'[1]

We must return now, after this digression, to the further consideration of types of 'things'. We have a whole series of entities exhibiting various degrees of 'thinghood': a flash of lightning, an atmospheric gas such as Argon, a pool of water, a stone, a crystal, the solar system, a living organism, a beating heart, a race of organisms, and a society of organisms, e.g. bees. A further examination of these very different entities will make clearer the notion of thinghood, and prepare the way for the biological concept of organization.

The flash of lightning has a very low degree of thinghood on account of the very brief duration of the event of which it is a knowable character. The atmospheric gas exhibits extreme permanence of property but lacks spatial boundary and sensible qualitative characterization. Definiteness of spatial boundary confers on the amorphous stone one characteristic of thinghood but this is counter-balanced by its contingency of form. In this respect the crystal has a superiority, and this appears to be an additional mark of thinghood. The next three examples introduce a further complication. The stone and the crystal are permanences without change, or such changes as they may exhibit are dependent on contingent relations to other things. But in the solar system, the beating heart, and the individual living organism we have things of which change is an intrinsic and not a wholly contingent character. Thus in this case we not only know an event as a thing, but we also know it as characterized by a certain *type of change of characterization*. There would therefore appear to be two modes of characterization: (1) representing differentiation of the *spatial* parts of an event, and (2) representing differentiation of the *temporal*[2] parts of an event. For example, successive temporal slices of the history of the crystal (assuming of course that its relations to other things do not change, e.g. it is not put into water) all exhibit the same spatial characterization and qualitative differentiation in virtue of which it is recognizable as a crystal of copper sulphate. But the successive temporal slices of

[1] *Principle of Relativity*, p. 38.

[2] This is more fully explained in Ch. VI., Sect. 6, pp. 299 and 304.

the heart are not the same: only certain ones will be comparable. There will be a certain minimum period or duration embracing the whole 'cycle' of changes, and this is repeated in successive periods. This, then, is an example of the second of the above two modes of characterization. Moreover this, like the crystalline form, is an *intrinsic*[1] character.

Now consider the individual living organism. This, like the crystal, exhibits a specific form, but it differs from the crystal and resembles the heart and the solar system in also manifesting the second type of characterization as well. What we call the life-history of the organism is simply the organism considered in its temporal extent, with its varying characterization: just as what we call its 'anatomy' is the organism considered in its spatial extension with its varying characterization. But the organism differs from the solar system and the heart in that its temporal differentiation is not rhythmical but *serial*. We can now summarize by making a table of types of solid things:

A. Those which are homogeneous as regards intrinsic temporal characterisation.
  (1) Those with contingent spatial characterisation: e.g. a stone.
  (2) Those with intrinsic spatial characterisation: e.g. crystal.
B. Those which are differentially characterized in their temporal extension:
  (1) Rhythmically, e.g. heart.
  (2) Serially, e.g. individual organism.

(Note that the mode of spatial characterization of the entities under B are to a large extent intrinsic, not contingent.)

We can now pass on to consider those entities which common sense would certainly not recognize as 'things' but which are substances in Aristotle's secondary meaning of that term. The smashed crystal is no longer one thing but it is still copper-sulphate and thus a *chemical* substance. Similarly the race to which an organism belongs is also a substance in this secondary sense although not a chemical one. How can we express the difference between these two examples of

[1] By 'intrinsic' I do not mean independent of everything else, but that its special peculiarities are not directly correlated with the surroundings, e.g. the rhythmical character of the beating heart is not dependent on the *rhythmical* character of surrounding events.

secondary substances? Here again our distinction between differentiation of spatial extension and differentiation of temporal extension will help us. The chemical substance has no *intrinsic* temporal differentiation, whereas the organic 'secondary substance' is intrinsically differentiated in its temporal extension. Moreover this differentiation is of the *rhythmical* type. An individual organism is a spatio-temporal part of the race to which it belongs, and exhibits serial change (most obviously in the developmental processes), and the race —the 'secondary substance'—consists of a succession of such individuals each of which repeats (approximately) the serial changes which characterized its predecessor. Thus the individual organism is the minimum temporal part of the secondary organic substance which exhibits one complete 'cycle'—just as there was a minimum temporal part of the heart. But in the latter case, the heart, the rhythms are repeated in one thing, whereas in the case of the organic race the rhythms are repeated in spatially separate (and also to varying degrees temporally separate) individual organisms. Similarly the chemical substance is spatially distributed among a plurality of individual things. But any changes it may manifest are contingent changes, except, as we saw, in the remarkable case of the radio-active chemical substances.[1]

Now it is extremely interesting to note that biologists have tended to identify what is permanent in the organic race, and thus exemplifies one of the ideas to which the category of substance gives expression, with either a perceptible part of the individual organism, e.g. the chromosomes, calling it 'idioplasm' or 'germ-plasm', or with hypothetical material particles after the manner of the physical sciences, e.g. the Mendelian genes. But if time is of the essence of the organism this may be expected sooner or later to lead to difficulties, and would appear to underlie the antithesis between preformation and epigenesis.

Another point of interest connected with this, although it belongs to the specific topics of Part II, is that according to the doctrine of evolution there must be another temporal differentiation of the race-event superimposed upon its rhythmical differentiation, namely a *serial* change. And a

[1] Unless, of course, all chemical substances do change but they are too slight for detection by a period of our observations.

fundamental problem of theoretical biology is the problem whether this serial change is contingent or intrinsic or both, i.e. partly intrinsic and partly contingent. Moreover being serial, but not repeated like the individual life-history, this process is unique, and this fact appears to have important methodological consequences as we shall see later.

The relation between the individual members of a chemical substance is clearly different from that between the individual members of an organic race. Every organism is related to one or more other organisms by the genetic relation. That is to say it is in spatio-temporal continuity (with continuity of character) with a part of one or more organisms which is a part of its own temporal extension, i.e. its life-history.[1]

So far we have chiefly been occupied with intrinsic change, and the biological reader has probably been protesting against my speaking of intrinsic changes in the organism, as though I had forgotten that organisms had environments, and as though their changes were purely intrinsic. Nothing of the kind was, of course, intended, but we must deal with one thing at a time and hope that misunderstandings will correct themselves as we proceed. We must therefore turn now to those regularities of process which 'crop up' again and can therefore be *recognized* as regularities or persistences but which are not intrinsic to one thing but involve, apparently, at least two things. Our type was the fall of the stone. This is treated primarily as a transaction between the stone and the earth. This is a typical example of a causal process. It used to be said that the 'attraction' of the earth *causes* the fall of the stone, unless there is a table in the way which is the *cause* of the stone remaining where it is. Or, to take biological examples, the application of a pin to the skin of a decapitated frog is said to cause, or be the cause of, the contraction of a muscle; or the injection of adrenaline into the blood-stream of a cat is said to cause, or be the cause of, rise in blood-pressure. The notion of cause seems to arise when we find that changes of one thing are regularly related to changes in another, when, that is to say, we find that there is a mutual dependence between them. We discover such relations chiefly by putting

[1] This will be elaborated in Part II., see below, p. 356.

things together experimentally and finding what constituents are essential, and what are contingent and can be omitted. Those constituents which, on successive occasions, are found to be essential are commonly spoken of as causes and effects. We must be careful to avoid speaking of repeating an event as the term is used here. Strictly speaking an event cannot be repeated because a given event is unique and a particular, and can never be again. To repeat an event would mean to put time back. It is impossible to repeat the same process again under the same conditions, because when we repeat a process in the ordinary meaning of the phrase time has marched on and the whole universe is different in the sense that new temporal parts have come into existence. What happens when we are said to 'repeat a process' is that we put certain things in certain relations and another event happens which may to a close degree of approximation exhibit the same mode of characterization in its serial changes as did the former ones. What persists and is repeated is the character of a complex of events, but the events themselves pass irrevocably.

But before we attempt to discuss the general nature of causal regularities, and the relation of the causal category to that of substance, it is first necessary to examine in more detail some actual examples of such regularities as they appear in biological science.

The introduction of a small quantity of adrenaline into the blood-stream of a cat is followed by a rise of blood-pressure. Here is a straightforward case of cause (a definite chemical compound) and effect (rise of blood-pressure). What experiment establishes is a definite correlation between the introduction of this stuff into the blood and a sudden increase in blood-pressure. The two events are definitely related in temporal succession. But the effect is analysable and is, moreover, one which is capable of being caused by other changes than the injection of adrenaline. What is actually observed is a change in the height of a column of mercury connected with the carotid artery of the animal. Many complicated events intervene between the injection of the adrenaline and the rise of the column of mercury. We therefore ask what happens in the general circulation between the introduction of the adrenaline and the rise of the mercury column. We divide up the original temporal stretch into a series of shorter ones each with a beginning (cause) and end (effect). More-

over the cause is also analysable : there is the prick of the syringe, the adrenaline, the fluid in which it is dissolved, etc. Experiment shows that of all these elements it is the adrenaline which is essential if the effect is to follow, since without it the other elements are not followed by the effect in question. Consequently the adrenaline is called *the* cause. But we cannot call the rise in blood-pressure *the* effect because other things may cause it. We may call it the remote effect, and the aim of the temporal analysis is to discover the more immediate effect.

As a second example we may take the case of the formation of starch in the leaves of the green plant. Consider a duration of, say, half an hour—a four-dimensional slab of nature. We attend to a part of that duration, namely an event which is knowable as a certain green plant which persists throughout the duration. We concentrate on one of its spatial parts, e.g. a certain leaf and on a still smaller part of it, namely its starch content. We find that the starch increases in amount during the half hour.

We proceed to make a scientific analysis of this process of starch increase. But we cannot study that particular example because it is past and gone. All we can do is to state a historical proposition : Once upon a time starch was formed in the leaf of a plant. But the plant endures and perhaps starch formation is still going on. We assume it is still going on, or we infer inductively that it is probably going on and now proceed to alter the 'conditions' to see whether starch-production depends on other things. We find that conditions fall into three groups :

(1) Those upon which starch formation depends—the essential conditions.
(2) Those which can be altered without altering starch formation.
(3) Those which are beyond our control, and about which we can therefore say nothing.

The whole process is, from the common-sense standpoint, a rather complicated one. No one feature stands out and the whole process requires rather elaborate methods and a considerable development of scientific knowledge for its study. Consequently we are not much tempted to call any one feature cause and another effect. We can much more easily think of the whole as one continuous process in which we can discriminate various contributory constituents :

a air between certain limits of

(3) Plant cells with *chloroplasts* containing chlorophyll.
(4) Starch.
(5) Oxygen.

Now all these constituents are complex and analysable. (2), (3), (4) and (5) are things, but (1) does not appear to be a thing, although the sun is a thing. Some of the things disappear in the process and other things appear, although there is no total change in weight, i.e. there is chemical change. Chlorophyll does not seem to increase or decrease but is an essential constituent. Other events may be involved but they are not attended to or not discriminated.

Thus a causal analysis of this process consists in finding the simultaneously present conditions which are essential. It is a spatial rather than a temporal analysis. But it is not supposed that the starch 'instantaneously' appears: there is every reason to suppose that intermediate compounds are formed. A further study of the process consists in a more detailed analysis of the various constituents and an attempt to assign to them their particular rôles in the process. Thus it is found that sunlight is not essential as such, and that some sorts of light are more favourable for starch formation than others. What sort of an existent is light? We relate it to a thing which is called its source but in the present case light of a certain sort suffices irrespective of the kind of thing which is its source. But it is not a substance in the chemist's sense because it cannot be weighed and persistence of weight is the chemist's criterion of a substance. If we say that light is a mode of vibration of the ether all we assert is that light has a periodic character, or is a periodic character of certain events, and we invent a hypothetical substance for it to vibrate in. But this merely represents a confession of our inability to think of vibrations unless they are vibrations of a stuff. The truth would appear to be that light is an entity of a kind for which common sense has no name and no concept, and we therefore try to assimilate it to other types of existents.

In all cases of causation in natural science we are dealing with total events with their two aspects of spatial and temporal extension. A scientific study takes the form of an analysis of such an event with an emphasis sometimes (as with the adrenaline) on its successive temporal parts, and

sometimes (as in the case of photosynthesis) on the spatial, i.e. it is an analysis of the simultaneously occurring constituents. Now it is evident that the terms 'cause' and 'effect' merely give emphasis to the more outstanding features in a continuous process. One such noticeable feature is called 'cause' and another which follows it is called 'effect'. These terms, therefore, do no more than express those features which, from practical importance or ease of discrimination, our attention fixes upon, but they are of no theoretical importance and may easily be misleading.[1] Thus when we say that adrenalin is the cause of a rise in blood-pressure we take a thing as cause and a change in an organism as effect. But in the concrete situation what we have is a duration the earlier slices of which contain an organism in a certain spatial relation with the perceptible stuff called adrenaline. In a later slice we have the spatial relations altered. In a later slice still the change described as rise in blood pressure characterizes certain temporal parts of the event which is known as the organism in question, i.e. this change characterizes certain periods of its history, and depends not only on some spatial parts but in varying degrees on all parts. For in an organism one part exists because it is a part and not as a self-subsistent thing. Thus the rest of the organism constitutes the so-called 'conditions' which are simply taken for granted because we are trying to narrow down as far as possible what has to be taken into consideration in investigating causal laws. Only when

[1] I therefore agree with Mach when he says: 'In speaking of cause and effect we arbitrarily give relief to those elements to whose connexion we have to attend in the reproduction of a fact in the respect in which it is important to us. There is no cause and effect in nature: nature has but an individual existence; nature simply is. Recurrences of like cases in which A is always connected with B that is, like results under like circumstances, that is again, the essence of the connexion of cause and effect, exist but in the abstraction we perform for the purpose of mentally reproducting the facts. Let a fact become familiar, and we no longer require this putting into relief of its connecting marks, our attention is no longer attracted to the new and surprising, and we cease to speak of cause and effect. Heat is said to be the cause of the tension of steam; but when the phenomenon becomes familiar we think of the steam at once with the tension proper to its temperature. Acid is said to be the cause of the reddening of tincture of litmus; but later we think of the reddening as a property of the acid.' E. Mach, *Science of Mechanics*, Eng. trans. 1893, p. 483. I am in agreement with the above in so far as it denies the existence in nature of causes and effects as 'distinct existences' as Hume expressed it. But I am not asserting that I believe causation to mean 'nothing but' regularity of sequence.

those laws are not upheld do we then turn to the conditions to explain the departure from them. We suppose that these conditions are unvarying unless the causal law is not upheld; to explain the deviation we then appeal to postulated deviations in the conditions. In other words, although we have not analysed the conditions, and perhaps know nothing about them, we assume, if we find regularities, that they are constant. When a discovered regularity is departed from we do not blame the causal law but suppose that it has been 'interfered with' by changes in the conditions. What we have to deal with then is an event with parts in mutual dependence upon one another. We can consider any part and try to determine its relations to other parts, the former being called cause and the latter effect. Meanwhile, however, there are other events of equal importance involved in the whole which are essential in that whole for the existence of the parts we are considering.

Again, when we consider successive marked features of an event we arrive at the notion of a 'chain of causation' but this is merely to concentrate on the serial or temporal aspect of the event and to forget the spatial or simultaneous aspect. The belief that cause precedes effect is founded on the fact that we only discover transeunt causal regularities by making an alteration in one thing and observing what change then appears in another. Where motion or transmission is involved there will always be a temporal interval. But clearly succession is no more especially characteristic of causation than of persistence without change, since both involve temporal passage.

I wish now to bring this discussion of causation into relation with the previous discussion of the category of substance. I have already urged (p. 183) that there are two modes of characterization of events in virtue of which knowledge is possible: (1) representing recognizable modes of differentiation of the spatial parts of an event; (2) representing recognizable modes of differentiation of the temporal parts of an event. The notion of the individual thing expresses, as we saw, persistence of spatial characterization with or without temporal homogeneity. In the absence of temporal homogeneity we can none the less discern in the history of the enduring thing a permanence of characterization in its *mode* of change—either rhythmical or serial, although the serial type requires, for its recognition, reference to other things characterized by it, since

unlike the rhythmical it is not repeated in, or does not characterize, the same individual thing, but only a class of things. For example, the serial process of development cannot be studied in one organism but necessitates the study of other specimens in the same species.

Now it seems clear that when we discover a causal regularity of the type we have just been discussing (i.e. one involving the mutual relations between a *number of things*) we are knowing *different* complexes of events as being characterized by the *same type* of serial change. In other words as exhibiting the second mode of characterization. But in this case the constituent events, not being mutually related so as to be knowable as parts of one thing, i.e. not having 'substantial unity', the manifestation of this mode of characterization is contingent upon their coming into the appropriate spatio-temporal relation, but once this relation is established on a given occasion, then the total event exhibits this mode of characterization (if the causal law holds). We then call such a recognizable character a causal law or regularity, and we are apt to attach it to the ingredient 'things' as a 'property'. For example, we discover the 'secondary (chemical) substance' called copper sulphate. We place a crystal in a vessel of water, the history of the crystal and water in their mutual relations is 'known as' '*dissolving* of crystal in water'. Such a character is always found to characterize a duration containing an event which is knowable as 'copper-sulphate crystal in water'. The solution of the crystal in water is one total event involving the water as much as the crystal, but we now speak of 'solubility in water' as a property of the crystal and of the latter as 'possessing' this property, and of the water 'causing' it to dissolve. We can, therefore, distinguish two divisions of the second mode of characterization of events: (1) temporal differentiation of an event which is also known as one thing, and (2) temporal differentiation of an event which can be discriminated into part-events which are not related so as to be knowable as 'parts' of one thing. This corresponds to the distinction between immanent and transeunt causation.

We can now bring what has been said about the categories of substance and causation together in the following way: both categories give expression to the discovery of permanences in nature—permanences of characterization amid

passing events. An event or complex of events may be *known as* either (1) an object or unchanging thing, or (2) the 'situation' (in Professor Whitehead's technical sense of the term[1]) of a causal law, i.e. as exemplifying a causal regularity. And this is the case where there is *change* in the mode of characterization, either (a) in one thing, or (b) involving two or more things. This can be expressed in tabular form as follows:

PERMANENCES IN NATURE.

1. Of spatial characterization without (intrinsic) change:
   a. Things or Objects (i.e. 'known as' objects).
      [i] Perceptual.
      [ii] Purely conceptual.
2. Of modes of change of characterization (i.e. temporal differentiation)
   b. Relating to one thing (immanent causal laws).
   c. Involving two or more things (i.e. transeunt causal laws).
3. Relations.

The last item is added because it is obviously required to make the list complete although it has not been specifically discussed. Also it is to be noted that causal laws also involve events which do not have perceptual objects related to them, as was the case with the events intervening between the source of light and the green leaf in our second example of a causal regularity in biology. Such events have no 'thinghood' and have not been dealt with in the above discussion. According to modern physics all we know about such entities is that they obey Maxwell's equations.

Our discussion of substance has chiefly been concerned with three of the ideas expressed in the 'classical' uses of the category which were mentioned in the beginning of this chapter, namely (1) the individual thing, (2) the class of things, (3) that which is (relatively) permanent. But there was yet another idea involved in many of the definitions—the idea, namely of *independence*. From the standpoint of natural science it is hardly possible to speak of anything as absolutely independent in the way contemplated by some of the older rationalistic philosophers. But we shall see later that the notion of relative independence is of interest and importance from the biological point of view. It hardly seems possible to speak of independent events because every event is related to every other event by the relation of 'extension'. It is also difficult to say anything about the mutual

[1] See *The Concept of Nature*, p. 147.

dependence of events and sensa because we cannot apprehend events apart from sensa, and we cannot apprehend sensa apart from events. For our own bodies are involved in the apprehension of sensa, and our own bodies are events. Moreover, on account of the continuity of events every event is 'involved' in a given perceptual situation. We shall return to this question of independence as a mark of 'thinghood' in Section 8.

The above discussion of causation, as far as it goes, occurred to me as the result of reflecting on certain biological puzzles (which will be discussed in Part II) in connexion with Professor Whitehead's doctrine of events. This doctrine appears to me to offer us (apart from its other merits) a means of dealing with biological problems which seem to be refractory to treatment in terms of the old theory of 'inert matter'. But what I have here written about causation seems to harmonize with the doctrine of events, and, on going through Professor Whitehead's books, it seems to be in agreement with his references to causation.[1] It is much to be desired that biologists should give some attention to the theory of causation.

In this section I shall try to say a little more about the application of causal analysis in the organic sphere. In the inorganic world regularities of sequence of the transeunt causal type are very often far from evident and have to be sought. Usually they can only be discovered by experiment. The same is true regarding causal relations between the organism as a whole and the environmental events, and also between one part of an organism and another. But in an organism there are also, as we have seen, regularities of sequence of various types of a sufficiently obvious nature to be easily recognized. In such cases it is not a question of discovering causal relations but of analysing the observed regularities. There is, for example, the serial change which characterizes the developmental period of an organism's

[1] See for example *Concept of Nature*, p. 146: 'It is evident that the ingression of objects into events includes the theory of causation.' Also Chap. XVI. on "Causal Components" in *The Principles of Natural Knowledge*.

history. Here we have to ask, not *whether* it is a case of regularity, but *how* does this regularity come about? on what does it depend? Similarly, there is no question of whether the rhythmical beating of the heart is a regularity of sequence: the problem is, how is it dependent on other events within and without the organism? We know in this case that the rhythmical character is not causally related to other rhythmical events in the organism because it continues even when the organ is completely removed from the body, and for that reason I called this rhythmical character an *intrinsic* one. At the same time we know it is causally related to other events both within and without in so far as the *rate* of beat is concerned.[1] Thus in this case we seem to be justified in saying that the rhythmical character of the change is immanent in the organ. The same appears to be true of what is called the 'respiratory centre' as well as of ciliary movement. It may be true (in part at least) of other rhythmical and serial types of change in the organism. Their rhythmical or serial character is not directly dependent upon rhythmical or serial processes in anything external to themselves, and in so far as that is the case they may be described as immanent processes. But this is not intended to imply that they are in no way causally related to other events, for the *rate* of many processes of this kind may be causally dependent on environmental conditions, e.g. temperature.

Now our notions of causation are founded upon transeunt causal processes between one thing and another—especially on our own pushing and pulling. Consequently the above regularities in organisms appear to us as extremely strange, and physiologists have devoted much attention to attempts at interpreting them in accordance with ordinary causal notions. But the possibility suggests itself that we may be misled in thus attempting to extend notions founded on one type of regularity of sequence to other types.[2] Speaking generally, it is found that most of the events going on in the organism are much less directly related to, or dependent upon, environmental events than is the case with inorganic things,

[1] Also the persistence of the heart is dependent on the rest of the organism.

[2] See P. Jensen: *Reiz, Bedingung, u. Ursache*, Berlin, 1911. A discussion of the concept of 'stimulus' from the physiological standpoint. He also protests against the usual view of rhythmical processes, e.g. on p. 51.

e.g. crystals. Moreover, this is true of 'higher' organisms to a greater extent than of 'lower'. And another point in connexion with the application of causal analysis to organisms which may be mentioned in passing is the fact that when once an organism has entered into a given causal regularity it is in some sense no longer 'the same' because it exhibits different changes on a subsequent occasion.[1] It is not suggested that this is a peculiarity of organisms, but it certainly makes the application of causal analysis more difficult.

We have seen that among the characters which a thing must possess if it is to be regarded as possessing a high degree of 'thinghood' are (1) length of duration, and (2) intrinsicness of character. But the notion of transeunt causation directs us to inquire into the mutual dependence of things, and we find that endurance is an aspect of mutual dependence. Some things are much more dependent for their existence on other entities than others are. An event which exhibits constancy of characterization amid varying conditions is known as a thing having a larger measure of thinghood than one which is less independent of such variations. Hence the nature of its transeunt causal relations with other things has to be considered in considering the persistence of things. Thus although the empirical standpoint does not entitle us to speak of absolute independence (as we pointed out in Section 6) we nevertheless distinguish different degrees of independence among things, and this will be the topic of the present Section.

In a sense there can be no explanation of the persistence of things. For we find that such attempts resolve in the long run into saying that a thing persists because it is persistent. We are told, for example, that an organism 'survives' because it is 'fit'. If we ask under what circumstances we are entitled to say that an organism is 'fit' we are told: when its characters or properties are such that it 'survives'. But if we cannot 'explain' persistence we can compare the modes of persistence of different things, and this is of importance for biology.

Natural science—particularly physical science—has largely been occupied with the investigation of causal regularities

[1] See below, p. 219.

between two or more things. But the type of regularity or 'permanence' exemplified in the persistence of one thing is of equal importance and interest in biology. It appears in fact to constitute the central problem. Perhaps it is partly for this reason that the 'habits of thought' characteristic of the physical sciences help us in biology when it is a question of causal regularities between one isolated process and another, but do not seem to offer so much assistance when we come to reflect upon the questions of the persistence of the whole. But the importance of this standpoint is brought home to us when we consider that the whole of the science of medicine is occupied with the problem of the persistence of an organism.

Moreover the physical sciences concern themselves but little with single things as such.[1] Physics is largely concerned with universal types of change, chemistry with universal types of stuff, but not with the persistence of single things. But in biology all this is changed. Whereas the objects of physical science are very varied in their degree of thinghood—some, like blocks of granite having certain features of it in a high degree, and others, like electro-magnetic fields, not having it at all—in biology the primary object and starting point of study is always an individual thing sharply distinguishable from other things. For this reason the antithesis between organism and environment seems to be quite unavoidable for biology and its problems will be genuine and fundamental ones. And it is interesting to note in passing how, now that physics and chemistry are coming more to consider individual things—e.g. individual atoms and crystals—the notions of persistence, organization, organism, etc., are beginning to intrude.

We have already distinguished three modes of persistence of one thing:

A. Without intrinsic change.
B. With intrinsic change.
  (a) Rhythmical.
  (b) Serial.

In the inorganic world the type most commonly encountered is type A, but types B (a) and B (b) are not absent. The solar system furnished us with an example of type B (a) and others are furnished by speculative physics. The radio-active elements were cited as illustrating type B (b). When

[1] Cf. the quotation from Prof. P. W. Bridgman below on p. 331.

we come to living things we find type B (b) characteristic of the individual organism, but rhythmical change is characteristic of the race, with the further complication that type B (b), serial change, is (in some cases at least) superimposed upon it according to the doctrine of evolution.

When, now, we take into consideration the relation of other things to one thing from the standpoint of the persistence of the latter, those other things are numbered among the 'conditions' of its persistence. Such conditions fall into three groups:

(1) Those upon which the persistence of the thing depends—the essential conditions.
(2) Those which are indifferent as regards the persistence of the thing.
(3) Those which are beyond our control, are conveying and about which we are therefore unable to say anything.

What are called 'natural' conditions form a system with broad unchanging or rhythmical features within which there are changes of minor extent—the fixed stars, the seasons, night and day, composition of the atmosphere, etc., until finally we come to the conditions in a room, or in a small spatial part of that room, where we reach a state of affairs which we can control, or which we believe we can control. We proceed to investigate causal regularities. We find that the endurance of things depends on their relations to other things. Things which endure under a great variety of changes in their surroundings are said to be 'independent' of those surroundings. But this expression does not mean that the thing is out of all relation to its surroundings, but only that such relations are not of a kind involving destructive changes in it. The thing's surroundings are then said to be 'fit' for it, and it is said to be 'fitted' to them. 'Persisting in a state' without change is equivalent, as we have seen, to exhibiting a property. For example, a certain mineral is insoluble in water, and its persistence in nature is simply an exhibition of this property. It is in relation to an environment, but that environment is not such as to lead to its dissolution. The persistence of a thing is thus dependent on the absence from its surroundings of conditions under which destructive causal processes would occur. We can discover by experiment what those destructive conditions would be, and we know that some things persist under all the conditions

that we are able to subject them to. Thus there are degrees of 'independence'. The history of chemistry represents a search for the most independent things—the elements, the less dependent things being regarded as composed of the elements. Again some composite things are more enduring than others, and are said to be chemically 'inert' or 'stable'. But this is not the only kind of composition. In general we find that there is a simple direct relation between changes in inorganic things and outside events. Absence of change in things of this type is an expression of chemical inertness, or stability. But this is, of course, no explanation of their persistence: it amounts to no more than saying that they persist because their properties are such that they persist.

Living organisms are also 'knowable as' being composed of elements in the chemical sense which are compounded into compounds of the unstable sort. But the organism as one thing is much more stable than many of the compounds into which it can be decomposed, although not so enduring as the elements. Thus we have to inquire into the grounds of biological endurance, and compare them with those involved in the persistence of inorganic things. When we come to the living things we find the same dependence on surroundings but the situation is complicated in various ways which require analysis. In the first place the organism is extremely heterogeneous spatially. It always has very diverse spatial parts. Moreover we know that those parts themselves are also diversified in their temporal extension both contingently and intrinsically. Some, for example, exhibit the rhythmical type of temporal differentiation. We know too that the persistence of the individual living thing may be dependent on the persistence of such changes, e.g. beating of the heart. Thus in studying the nature of organic persistence we have to take into consideration not only diverse events in the environment but also diverse events in the organism itself. Moreover organic parts have the peculiarity that they do not exist 'in nature' independently of the organism of which they are parts. This is not true, of course, of *chemical* elementary parts, and other reservations are necessary when we come to details in connexion with the problems of Part II. And, since organic parts are themselves diverse, questions involving the mutual relations of the different kinds, and the different degrees of thinghood of certain parts (e.g. cells) will also arise.

Then there are complications introduced by the fact that organic persistence is of the serial type. We find that this serial change is of the essence of organic persistence because if it ceases the organism ceases to endure. The very being of an organism depends on the continuance of serial change of it as an individual thing, and this again depends, as we have noted, on the persistence of rhythmical or other changes in its parts. The question of 'organization' when we come to discuss it will thus resolve itself into the mode of spatial and temporal differentiation in the characterization of those events which are 'known as' individual living things. It is evident that organic persistence from the point of view of the individual, is a highly complicated process but it is not suggested that this is not also the case with some inorganic things.

We will now consider racial persistence a little. The race is constituted by a series of individuals each of which exhibits serial change, such serial change being essential to each individual. The starting point of each individual is always a part of another individual. Thus a single individual throughout its history is a temporal part of a race, and the persistence of the race is therefore dependent upon the serial changes of its component temporal parts, and on the occurrence of the process of separation, division, or reproduction. It is this latter process which makes possible the existence of such an entity as a race. A race may be defined as a persistent entity constituted of individual things, each of which things exhibits serial change of approximately the same type, and is such that it has arisen from a part of another individual, i.e. stands in a genetic relation to it. A number of such individuals whose histories are all contained in approximately the same duration constitute what is called a 'generation'. But this term can also be understood to mean all the individuals constituting the immediate derivatives of the union or unions of a given pair (in the case of sexual reproduction)—a definition which could hardly be used except in regard to races in captivity and under observation. The genetic relation is *asymmetrical* (i.e. if x is derived (in the genetic sense) from y, then y is not derived from x), *transitive* (i.e. if y is derived from x and z from y, then z is derived from x), but *not connected* (because if x and y are two members of a race it is not true that either x is derived from y or y is derived from x, since neither may be derived from the other), consequently the genetic relation

does not generate a series in the strict sense. It would only generate a series in an ideally simple race constituted of individuals each of which was such that it laid only one parthenogenetic egg and perished immediately afterwards!

The process of division above referred to through which reproduction is effected is not confined to the organism as a whole, but extends to some of its smaller parts. And the possession of parts capable of division in the biological sense without loss of their intrinsic characterization appears to be a feature peculiar to organisms. In the higher organisms it is not merely subservient to reproduction but is also involved in some processes requisite for the persistence of the individual. Moreover division implies a process of elaboration of that which is divided, at least in so far as organisms are concerned, and consequently this necessity renders the organism utterly dependent on its surroundings for its persistence.

Now we saw that the persistence of a thing without intrinsic change is an expression of the absence from its environment of other things, relation with which would involve changes leading to its destruction. Such a thing persists because 'dangerous' changes do not occur in its environment. But suppose a thing were such that changes in its environment were related in such a way to changes in it that the latter changes resulted not in its destruction but in its further persistence, then clearly it would manifest a new type of persistence which has not yet been mentioned. It would be independent in a new way—not through *absence* of change, but *through* change. It would be characterised by a new mark of thinghood. Such modes of change in the thing might be intrinsically determined in the sense that, for example, there might be a certain set of external changes a,b,c,d, . . . etc., and a corresponding set of changes of the thing—A,B,C,D, . . . etc., so that when b occurred B would occur in relation to it. But suppose the thing were such that *any* external change resulted in an 'appropriate' change in the thing, then, apart from intrinsic determination of the length of its duration the thing would be eternal.

Now it is a characteristic feature of living things that they do manifest in various degree this type of persistence—this further mark of thinghood. Moreover the 'higher' an organism is the greater is the variety of external changes to which it is capable of making 'appropriate' changes or

'responses'. And those lines of evolution which are spoken of as 'progressive' appear to represent a gradual development of greater and greater independence of environmental change. When we come to man what is called the 'possession of mind' seems to signify an approximation to independence of any such changes,[1] since it brings with it the possibility of prevision. But the endurance of the organic individual (at least in the higher organisms) is intrinsically determined—hence the possibility of life insurance. Whether racial endurance is also intrinsically determined nobody knows.

[1] Cf. the passage from Descartes quoted above on p. 51.' for while reason is an universal instrument that is alike available on every occasion, these organs, on the contrary, need a particular arrangement for each particular action.'

CHAPTER IV

Demands, Postulates, and Subjective Factors in Knowledge

Mr. W. E. Johnson defines a postulate as :

> ' a proposition that is assertorically and not merely hypothetically entertained but yet is adopted neither on the ground of intuitive self-evidence nor of inductive confirmation. More positively, a postulate is framed in terms not given in experience, and these enter even into the instantial propositions which are problematically universalised by induction.'[1]

Similarly Dr. Broad, speaking of scientific ' postulates ' which are not self-evident, in contrast to the laws of logic or ' axioms ' which are self-evident, writes :

> ' Of these scientific postulates we may say (a) that they cannot be *disproved*, any more than they can be proved, and (b) that it is practically more advantageous to act as if we believed them than to act as if we disbelieved them. There is no *logical reason* for believing them, but there is a *practical motive* for acting as if we believed them.'[2]

Regarding demands Professor L. J. Russell[3] speaks of them as constituting a ' set of generalizations which guide the investigator ' which are ' tenaciously held to in the face of contrary evidence ' as though they expressed ' something in man's attitude to the facts rather than . . . the mere facts themselves '. He says that they resemble hypotheses in so far as they are anticipations—' they say more than nature tells ' but they are distinguished from hypotheses ' in being more truculent. They are challenges. They insist that things must be so, and challenge all comers to show, if things are not so, how this is possible.'

[1] *Logic*, Part III., Cambridge, 1924, p. xviii
[2] *The Mind and its Place in Nature*, London, 1925, p. 510.
[3] *Proc. Arist. Soc.*, Vol. XXV., 1925, p. 61.

In what follows I shall not attempt to draw too fine a distinction between postulates and demands and other elements involved in 'principles of systematization' of a similar kind. All such elements as I have collected together in the following six groups have certain features in common in virtue of which they can be discussed together. They are all capable of 'furnishing a motive for research', are in some sense a priori, are liable to be used blindly and uncritically, and, as we shall see, are of great importance for the study of the biological antitheses.

I. Desire for monistic interpretations.
Demand for continuity.
Refusal of arbitrary breaks.
Attempt to reduce all natural science to physics.
Demand for persistence and identity.
II. Demand for simplicity and economy in explanation.
Occam's razor (so-called).
III. Desire for atomistic interpretations.
IV. Demand for verification.
V. Desire for 'stuff.'
VI. Demand for predictability, unequivocal determination; universality, 'necessity', 'uniformity of nature'. The postulate of the validity of inductive generalization.

Speaking genetally we may say that all these elements which are or have been involved in the systematization of scientific knowledge are partly suggested and supported by experience otherwise they would not have persisted in science. But they are none the less hypothetical in nature and we have to bear constantly in mind from our present point of view that the experimental sphere in which they have grown up has chiefly been that of common sense and physical science. Outside these spheres they have no claim which can be laid down a priori or which would justify us in using them blindly. If we convert our demands and postulates into universal a priori truths and not 'suffer them to be doubted of' we shall be saddling science with a creed and, according to Huxley, science commits suicide when it adopts a creed. Or, as Karl Pearson expresses it:

> 'One of the most fatal (and not so impossible) futures for science would be the institution of a scientific hierarchy which would brand as heretical all doubt as to its conclusions, all criticism of its results.'[1]

For the present we are concerned not with results but with

[1] *Grammar of Science*, p. 55.

the foundations upon which they are based. But foundations are as much in need of critical scrutiny from time to time as results, and that for two reasons: first because being foundations so much depends upon them, and secondly, because being foundations they are liable to be buried and forgotten. If we employ them ignorantly and dogmatically we shall no longer be treating science as a search for truth but we shall be insisting that the truth shall conform to our wishes. The value of these elements in the systematization of knowledge is purely methodological; science has no concern with employing them metaphysically. Our orthodox traditional way of thinking in biological science is not an expression of a pre-established harmony between thought and things, but a mode of thought which has grown up in adaptation to a certain sphere of experience over a certain limited scale of magnitudes. How far it has value outside these limits can only be determined by trying them. The celebrated remark of Hobbes about words will apply equally to demands and postulates.

In this chapter I shall not attempt a detailed discussion of the above six groups but will merely draw attention to the general characters of each in preparation for what is to follow in Part II.

Group I. This forms a fairly compact group. The monistic ideal has usually taken a metaphysical form as a demand for a monism of *stuff*. It is typically represented in the point of view commonly referred to as that of the 'billiard ball universe'. For example, it would be realized if the world could be exhaustively represented as constituted by a system of structureless particles only differing from one another numerically and existing at points in a succession of instantaneous moments of time. Such a scheme has of course only been used in relation to certain limited and abstract problems for scientific purposes. The atomicity of this notion had to be compensated for by the doctrine of the ether. But to-day the assimilation of space and time and the resulting concept of the event has put quite a different complexion on the whole situation. Consequently the notion of stuff itself requires revision. From the standpoint of a given science the question of monism only arises in connexion with questions

about the meaning of its fundamental concepts. That is to say it is a question of what is to be considered as fundamental in a given sphere of experience for the purpose of systematising knowledge about it, and this is primarily determined by the facts of the case, and not by metaphysical considerations. And at the present moment no science works with a monism of stuff.

The desire for monism, in a special form, is of course operative in the objections to vitalism or certain forms of vitalism in biology. In the vitalism of Driesch an appeal is made to a special kind of *agent* which is only operative in living organisms. This appeal violates so many scientific demands that it is hardly surprising that it should have so few supporters. Probably the average biologist never troubles to analyse his objections. His 'scientific intuitions' revolt against it. He 'feels in his bones' that something is wrong, and that is enough. I am conscious of similar vague unanalysed objections in myself. But feelings must not be mistaken for arguments, and vitalism cannot be refuted merely by 'expressing your feelings', although many people seem to suppose that it can, and consequently this antithesis between mechanism and vitalism is rarely in the proper sense of the term *discussed*. On the other hand the so-called proofs of vitalism are not proofs of vitalism at all, but *disproofs* of certain forms of mechanism. The best way out of all these difficulties is not to appeal to imperceptibles—whether entelechies or matter—but to return constantly to concrete experience and keep an open mind with regard to demands and postulates. It is not a question of whether we should have one or two ultimate imperceptibles in natural science, but of whether we should have any at all, and if so what *reason* can be given for such hypotheses.

What does the demand for continuity mean? Professor Whitehead speaks of a strong point of the old materialism as being the demand 'that no arbitrary breaks be introduced into nature, to eke out the collapse of an explanation'. The collapse of an explanation usually results from the refusal of brute facts to fit into an existing scheme which has successfully accommodated others. In such a case there are two alternatives: either the existing scheme can be modified in order to embrace the brute facts, or it may be possible to analyse the latter further into elements which will be amenable to treatment by the existing scheme. If neither of these alter-

natives is possible then the brute facts will have to be dealt with in a provisional scheme of their own. No one is in a position to *introduce* arbitrary breaks into nature.

The notion of continuity has played a great part in discussions involving evolution. For example, because 'consciousness' is in some sense an attribute of human beings, and because human beings are genetically derived from other animals according to the theory of evolution, it is argued that in order to preserve 'continuity' we must also ascribe consciousness, however 'dim', to lower animals. Thus this argument interprets continuity to mean not merely *genetic* continuity of the human species with some prehuman race, which has a genuine basis in fact, but interprets it to mean that a given *characteristic* in a late stage must have been present in some form from the beginning. 'Consciousness' is treated as a kind of indestructible stuff which cannot arise *de novo* but can only undergo modifications.

Similarly, Huxley adopted Descartes' arguments that animals were automata, and then urged the above notion of continuity in favour of the view that they were also conscious. He then argued forward to man that he also was a conscious automaton. Continuity thus cuts both ways and forbids the possibility of novelty. Such an argument appears to rest on the assumptions (1) that if some animals are automata *all* animals are automata; and (2) if some animals are conscious *all* animals are conscious. But are we really justified in passing from some to all in this way? All the theory of evolution states is that in the course of the earth's history the individual organisms constituting certain genetic series have gradually undergone change. The only use it makes of the notion of continuity is in the assumption that the genetic succession along each line has been continuous in the case of all organisms alive at the present day. All such arguments as the above from continuity appear to be reversible and it is a purely arbitrary affair to determine in which direction we choose to use them.

The demand that all natural science should reduce to physics also belongs to this group. It may be urged either on metaphysical grounds, e.g. on the basis of some monistic theory, or on methodological grounds. This will be discussed in detail in Part II when we come to the antithesis between mechanism and vitalism.

The demands for persistence and identity are called by Professor L. S. Russell metaphysical demands, and in the history of science they have usually taken a metaphysical form. Wherever persistence is discoverable it tends to be interpreted as persistence of stuff. Persistence of mass and the kind of quantitative persistence represented by the concept of energy have both been interpreted as persistence of stuff in spite of change. We see the same tendency in theories of 'heredity'. The persistent mode of characterization of a race of organisms is interpreted as a consequence of the persistence of a kind of stuff in their germ-cells. Explanatory entities in natural science thus always tend to take the form of enduring things which are supposed to persist unchanged and *account for* the changes which are actually observed. Change has therefore been regarded as superficial or illusory, and this is an instance of the curious tendency to regard anything that has been explained as 'unreal' or less real than that in terms of which it is explained. What is observed is appearance and what is not observed but is assumed or inferred is regarded as reality. This happens in metaphysics as well as in science, or, to be correct, it happens in other systems of metaphysics than those commonly associated with natural science. They differ only in the kinds of entities which are regarded as persistent but both agree in regarding the observed world as illusory.[1]

Group II. The demand for simplicity in explanation has fulfilled a valuable function in directing attention to the easiest tasks which were within the competency of each age. This demand figures in the methodological rules which Descartes laid down for himself in the *Discourse on Method*. It is this demand which is given so much emphasis in the writings of Mach and his admirers, until it seems to overcrowd all the others and the whole pursuit of science is represented as simply a device for the saving of mental labour. 'Seek simplicity' says Professor Whitehead, 'and distrust it.' But those who make the *Sparsamkeitsprincip* their chief guide would have us omit Professor Whitehead's cautious addition.[2]

Some points of interest in connexion with this demand

[1] The use of the notion of identity in natural science is discussed by M. Meyerson in *De l'explication dans les sciences*, Paris, 1921.

[2] For an example in biological literature see the article by O. zur Strassen in *Kultur der Gegenwart*, Teil III., Abt. iv., Bd. I, p. 87.

are brought forward in the following passage from Mr. Bertrand Russell:

> 'What is surprising in physics is not the existence of general laws, but their extreme simplicity. It is not the uniformity of nature that should surprise us, for, by sufficient analytic ingenuity, any conceivable course of nature might be shown to exhibit uniformity. What should surprise us is the fact that the uniformity is simple enough for us to be able to discover it. But it is just this characteristic of simplicity which it would be fallacious to generalize, for it is obvious that simplicity has been a part cause of their discovery, and can, therefore, give no ground for the supposition that other undiscovered laws are equally simple.'[1]

The demands for simplicity and economy give expression to a characteristic which the intellectual activity shares with the aesthetic. We experience intense satisfaction in reducing a muddle into an orderly and consistent scheme, and the maximum achievement with the minimum of means is part at least of what is aimed at by good 'style' in all branches of art as well as in proving mathematical theorems, and the affinities between the intellectual and the aesthetic activities by no means end here. But it cannot be assumed that nature will invariably conform to our aesthetic tastes any more than to our metaphysical wishes, and unless science is to be a purely arbitrary affair we must be prepared to relinquish our tastes in favour of the truth if occasion to do so should arise.

Group III. The actual experimental procedure of natural science is to a large extent analytical, and this fact seems to have led people to suppose that its mode of thought must also be analytical and they are therefore misled into making a sharp and highly artificial boundary between what they call 'science' and 'philosophy'. Appreciating as they do certain of the difficulties of a purely analytical procedure those who hold this view seem to suppose that it is so rooted in scientific thought that the latter can not be entrusted with the task of interpretation except for purely methodological purposes. Consequently the real business of telling us what science means falls to philosophy. But as I have urged the division between investigation and interpretation falls within science itself. It is only when a scientific theory becomes widely generalized that it becomes the concern of philosophy, and until such a degree of generality is reached it is surely the business of those who are in touch with the data to discover the appropriate

[1] *Scientific Method in Philosophy*, Oxford, 1914, p. 8.

concepts required in any particular field. But at the present day each of the major divisions of natural science has become so subdivided that there is a wide and important gap between the special knowledge of an investigator in a limited field in a given branch and the widest generalizations of the science as a whole which are of interest to philosophy. This gap should be filled by intermediate generalizations based upon a synthesis of the results of the special sub-divisions, and this would require more knowledge of the general results of all the special branches than either a specialist on the one hand, or a philosopher on the other, could be expected to have. Consequently it seems fallacious to make a sharp separation between science as investigation and philosophy as interpretation in the way contemplated above.

Atomistic notions are the outcome of a purely analytical mode of thought. They have been so fruitful and important in many branches of natural science that we seem entitled to speak here too of a demand. I have already called attention to some features of the analytical procedure of thought and of the notion of 'units'.[1] We can 'understand' an entity by analysing it into parts which are regarded—provisionally at least—as simple and with properties which are taken for granted. The first important application of this device to biology was in the so-called 'cell-theory'. This theory shows very clearly some of the characteristic features of the 'life-history' of scientific theories. In the first place, when once the central notion was grasped it developed very rapidly, and was very widely generalized. This generalizing process was carried out with great enthusiasm, with its accompanying intolerance of criticism, and was also accompanied by a good deal of metaphysical speculation—giving it an added zest—although these speculations were largely irrelevant to the scientific theory itself and were by no means a necessary consequence of it. Certain difficulties inherent in the theory from the start, which were brushed aside in the first flush of enthusiasm, have remained behind and slowly worked their way to recognition—although this process has been impeded by the glamour still glowing from the early dogmatic days of the theory.[2] Moreover this theory—like other atomistic ones

[1] See above, pp. 133, 315 and 321.

[2] The above description would apply also to the history of the doctrine of natural selection.

in biology—was hailed as providing biologists with 'units' which were to work the same wonders in biological science as atoms and molecules had done in chemistry and physics. The consequence has been a controversy of the kind so typical of biological science. When difficulties were pointed out in the use of the expression 'aggregate of cells', and when caution was urged regarding the extension of the cell-concept in certain directions an 'antithesis' arose between the so-called 'elemental' and 'organismal' views of the organism. One side clung to its 'real' units and denied that the organism was a unity, and the other side urged that the organism was more than 'the sum of its parts' and proceeded to inquire what the 'more' might be. The result is often an array of 'controlling agents' taking either a vitalistic or a materialistic form. We appear to have a deeply rooted desire for 'controlling centres' when difficulties of this kind appear—founded perhaps very largely on our preference for solid things. We find satisfaction in 'units' because they satisfy this craving for 'tangibility' and when the original unity from which we started seems to vanish like a pricked bubble on analysis we then proceed (if we reject what is considered to be the only alternative, viz. vitalism) to confer on *one* of the parts 'properties' in virtue of which it is believed to supply the missing 'control' through which the lost unity is to be restored. In this way we reach the 'pontifical cell' in the brain and similar notions. The history of atomistic notions in both psychology and biology exhibits many similar features. In both cases the advantages are confronted with difficulties which are met with answers of much the same type. In sociology the doctrine of the 'general will' is, perhaps, a device of a similar type to supply a 'unifying principle' which is believed to be required by observed facts and not discoverable in the facts. Now all these difficulties and controversies seem to result from our treating 'units' too concretely and refusing to take the *relations* in which they are embedded sufficiently seriously. This suggestion must be worked out in more detail when we come to the biological antitheses in Part II.

IV. The Demand for Verification. The demand that theories and explanatory entities should be 'verified' or 'verifiable' has been a strong point in natural science. It offers a means of deciding between what would otherwise be mere opinion. It constitutes a *differentia* which distinguishes

natural science as the exploration of the actual from philosophy which embraces also the possible

But unfortunately verification is not quite such a simple straightforward affair as it is sometimes supposed to be, and the word has more than one meaning. In one sense it may simply mean checking a judgment of perception. If someone says he has seen a sheep with two heads in a circus you can, if you are sufficiently interested, pay a visit to the show and satisfy yourself that this report is true. But when we speak of verifying a scientific hypothesis we usually mean something much less simple and straightforward than this. It does not mean looking to see whether the hypothesis is *true*, but looking to see whether the consequences which can be deduced from it are not contradicted. i.e. whether consequences which should be observed *if* the hypothesis is true *are in fact* observed. If they are observed then the hypothesis is commonly said to be verified, but this does not mean that it is *true*, it may not even mean that the probability of the hypothesis is increased. What usually happens is that the hypothesis suggests novel experiments or other observations and this frequently leads to a discovery, and such discoveries of course remain as data whatever may become of the hypothesis. This, then, is one merit that can be claimed for hypotheses but is this the only significance that can be attached to the demand for verifiability? Another point is that this demand excludes any hypothesis about a subject matter which does not admit of being tested—any hypothesis which is too sweeping in its scope to offer any foothold for an empirical test. This objection applies of course to demands and postulates themselves but these are admitted for methodological reasons *prior* to particular investigations. An hypothesis which cannot be tested to the above extent against perceptual experience, i.e. one which does not have deducible consequences for perceptual experience is thus either a methodological postulate or it is useless for scientific purposes. This appears to be the essential feature of the demand for verifiability. Professor Whitehead calls it the 'narrow gauge' by which a scientific doctrine is tested. He says:

> 'Only one question is asked: Has the doctrine a precise application to a variety of particular circumstances so as to determine the exact phenomena which should then be observed? In the absence

> of these applications beauty, generality, or even truth, will not save a doctrine from neglect in scientific thought.'[1]

It seems, then, that the demand for verifiability is placed in the very forefront by the scientific investigator—even before truth and simplicity. Simplicity is only operative when there is a choice of hypotheses all of which equally satisfy the demand for verification. And as regards truth the following remark of William James is interesting. Speaking of the 'nervousness' of the observer 'lest he become deceived' James says:

> 'Science has organized this nervousness into a regular technique, her so-called method of verification; and she has fallen so deeply in love with the method that one may even say she has ceased to care for truth by itself at all. It is only truth as technically verified that interests her. The truth of truths might come in merely affirmative form, and she would decline to touch it.'[2]

This is undoubtedly one of the many factors which help us to understand why vitalistic notions are so unanimously rejected by laboratory biologists, and the operation of this demand is well illustrated by the attitude of physiologists to the Bowman-Heidenhain theory of the renal function. Referring to this theory Cushny writes:

> '. . . such a nebulous statement of the renal function, while it offers a facile explanation of all possible observations, in reality explains nothing. As a defensive position it is impregnable, but it offers no point from which advance may be made.'
>
> '. . . every failure in the attempt to correlate an observation on renal secretion with other processes arising from the operation of known forces has been hailed as an argument in support of specific secretion, which really should only be resorted to when all other possible explanations can be definitely excluded, and this is seldom the case.'[3]

There are several interesting and very important methodological points in these passages. If the theory of Heidenhain were true we could understand why it could not be refuted, but the real sting in the objection to it lies in the fact that 'it offers no point from which advance may be made'. It is too successful, too complete. It comes in too facile a form. Consequently we have an illustration of the demand that an explanation should not bring the process of investigation to an end, which I have already called attention to as operative

[1] *The Principle of Relativity*, p. 3.
[2] *The Will to Believe*, London, 1897, p. 21.
[3] *The Secretion of the Urine*, 2nd edit., London, 1916, p. 50.

in the methodology of Galileo.[1] Thus a scientific hypothesis is required to hit a happy medium between being too successful, too all-embracing and thus capable of meeting all observable data, and being removed from verification altogether—which in practice amounts to much the same thing. But there is a second objection in the present instance: the theory appeals to 'specific secretion'. It is the almost universal practice of physiologists first to borrow a ready-made hypothesis from physics or chemistry and to apply it to the problem in hand. If it does not fit modifications are made which suggest further experiments. If these do not verify the hypothesis so amended then 'specific secretion' or some corresponding stop-gap is resorted to 'to eke out the collapse' of the explanation. Curiously enough this appears to be what Cushny himself does in the present instance, and it is therefore difficult to see how, at bottom, his procedure differs from that of Heidenhain. Thus he says:

> 'No one now attempts to explain the secretion of the urine by the known physical forces, yet in any consideration of the function of the kidney these forces must be taken into account, partly as acting in the same direction as the vital activity of the cell, partly as offering a resistance which it has to overcome.'[2]
>
> 'The concentration of a solution in its passage through a membrane demands the consumption of far more energy than is supplied by the known forces in the kidney, and the concentration of the urine has therefore to be attributed to the vital activity of the cells. The nature and method of action of this vital force is unknown, but it may be conceived as wafting the molecules which diffuse into the cell through its network somewhat in the way the cilia carry forward particles of dust that fall on them.'[3]

The difficulty which is presented to the usual way of approaching these questions is that the supposed 'vital force' is believed to work against a pressure of 35 atmospheres. It is remarkable to find a twentieth century physiologist using such expressions as vital force and vital activity, and another feature of interest in the above passages is the extreme simplicity of the thought, which is obviously of the 'mechanical model' type so familiar in the physics of the last century, and showing as it does how the notion of 'force' is still used in biology. It seems that the modern theory of renal secretion satisfies the monistic demand and that of 'reducibility' no more than Heidenhain's. And Cushny himself concludes by saying that the Modern Theory 'appre-

[1] See above, p. 39. [2] *Op. cit.*, p. 31. [3] *Ibid.*, p. 33.

ciating the inadequacy of the known physical forces, supplements them as far as is necessary by the "vital activity" postulated by Heidenhain.' But surely this procedure is no better than that of the vitalists. It is appealing to an arbitrary break in order to eke out the collapse of an explanation. A physicist who found such a bad fit between fact and hypothesis would reject the hypothesis rather than continue to use such a makeshift as this. The obvious thing to do would appear to be to construct an hypothesis suggested by the data observed rather than to borrow an ill-fitting hypothesis from physics in this way. But it never seems to occur to a physiologist to try to make a genuine biological induction. He seems invariably to take it for granted that physics has some ready-made hypothesis waiting for him to use. What does Cushy mean by 'all other possible explanations'? On what grounds is it always assumed that the laws of the inorganic world as worked out there will, as such and without modification, hold in the organic sphere? The reason seems to be simply that physiologists assume this without realizing that it is an assumption. It also has the advantage of saving the labour of thinking. In Part II *reasons* will be brought forward for believing that this is an unsatisfactory way of proceeding, and if only the belief that vitalistic assumptions are the *only* alternative were not so prevalent, physiologists might be more encouraged to stand on their own feet and use a genuine inductive method.[1]

To return, in conclusion, to verification: a scientific doctrine begins life as a hypothesis. If with the progress of investigation it remains uncontradicted, and if it leads to the discovery of fresh fields of fact increasing both in range of generality and in variety of cases and still remains uncontradicted, then it is usually called a theory. Finally it may become so universally adopted that no one either criticizes or doubts it, and it may then even be referred to as a 'fact'. For example, it is not uncommon to find the theory of evolution nowadays spoken of as 'the fact of evolution'. But of course from a strictly logical point of view a hypothesis does not become any the less hypothetical by being successful, it only

[1] The same difficulty arises in connexion with the regulation of the passage of fluids through the skin of Amphibians, see E. F. Adolph: *The skin and the kidneys as regulators of the body volume of frogs*. Journ. Exp. Zoology, Vol. 47, p.1., 1927.

becomes more probable, and the change in name from hypothesis to theory and finally to 'fact' is really only an index of increasing probability. Clearly the term 'fact' is used in science in a purely relative way since even a bare record of a direct observation conceals an enormous amount of generally accepted assumption which for the purposes of the investigator is forgotten, simply because he has no occasion to recall it. And between a fact in this sense and one in the sense in which the theory of evolution is sometimes called a fact there is no sharp or absolute boundary, since all is hypothetical from the logical standpoint. Such a passage is a gradual descent along a curve of decreasing probability.

V. The Desire for Stuff. Explanations in natural science often take the form of causal description in terms of stuff with various assigned properties. This is an extremely powerful tendency, and one which easily becomes transferred from the notion of stuff in a genuine chemical sense to other entities which enter biological thought. Attention has already been drawn to the way in which scientific concepts always tend to be interpreted in this way. Energy, for example, seems almost always to be conceived in this way in biological literature as is illustrated in the following passage: 'The human skin excretes energy just as the kidneys excrete matter.'[1] And this recalls the celebrated saying attributed to Cabanis that 'the brain secretes thought as the liver secretes bile'. Thus 'energy' and 'thought' as well as 'matter' all tend to be conceived as stuff. In the course of the history of science ether, electricity and heat have all been treated in this way. Thus we have to deal with a deeply rooted mode of thought which deserves attention. The notion doubtless has its prototype in the common-sense instances of stuff such as water, oil, etc. The favourite example of the common-sense thing is the solid, but this is extended in two ways. In the first place the discovery of the recurrence on different occasions of the same kind of thing leads to the notion of a general stuff out of which they are *made*, and the further generalization of this way of thinking leads to the notion of one or more fundamental stuffs out of which all things are made. The notion of 'making' and 'what things are made of' plays a great part in the thought of children. A second direction in which the

[1] Haldane (J. B. S.) and Huxley (J.), *Animal Biology*, Oxford, 1927,

notion of the solid stuff receives extension is one which enables it to embrace fluids and gases, and it is in this form that the notion of stuff has been employed in order to cope with 'imperceptibles' such as ether, energy, heat, 'thought', etc. It will be convenient to refer to all explanations which use the notion of stuff as 'materialistic', but without, of course, thereby implying any necessary reference to the metaphysical theory known by that name.

There can be no question about the utility of the notion of stuff, the only question we need to consider is how far we ought to allow our thought to be tied down by the use of the notion, and what is its precise meaning and ultimate analysis. Let us consider as an example a lump of iron. What does it mean to say that this is a lump of stuff? Our epistemological discussions in Chapters II and III have already furnished us with at least a partial answer to this question. Whatever I know about that lump of iron I know either (1) by looking, touching, feeling, smelling, hearing or tasting it, or (2) by observing what happens when a similar lump is placed in various relations with other things, or (3) by believing the reports of other peoples' experiences. All this resolves itself into 'knowledge about' the entities which are 'knowable as' iron. What I see when I look is not the same as what I feel when I touch, but I subscribe to the belief that all these sensa are related in a systematic way to a spatial volume during a period of time. Also I note that none of these sensa nor the happenings (i.e. changes in the correlated sensa) which I observe when the iron is put into relation with other things and so exhibits its properties can rightly be represented as standing in a simple two-termed relation to what is known as the iron, and consequently I cannot say that they are simply properties *of* the iron since they can with equal right be regarded as properties of the 'other things'. They are characters embedded in the actual concrete occasions of observing, and the iron is an abstracted recurring feature of such situations. The lump of iron that I am said to see as a common-sense thing being thus an abstraction cannot be thought of as a simple 'real' constituent existing as such in the world of nature. As a simplification of experience it has an unquestionable value, but its very simplicity constitutes a pitfall if it is interpreted ontologically. To call it a lump of stuff or matter is but to take a further step in abstraction.

I have followed the tendencies of modern thought in using the term 'event' for the spatio-temporal elements in the situation in which I am said to be 'seeing a lump of iron' because modern thought requires that spatial and temporal elements in the situation should not be separated. And it is just this demand which makes the notion of persistent self-identical stuff so unsuitable. Moreover it is in this sense that modern physical science can be said to be no longer 'materialistic'.[1] This does not mean that it is becoming 'mentalistic' or 'spiritualistic' but simply that it no longer makes use of the notion of stuff. Neither does it mean that biology will not go on using the notion of stuff, but simply that it will in doing so no longer be basing itself on physics. It will have to cease using it uncritically and metaphysically as an ultimate account of 'reality', but will employ it rather with the understanding that it is a provisional methodological device, and thus it will be able to use it more intelligently and less dogmatically. Accordingly if biological phenomena present themselves which cannot be treated adequately by means of the notion of stuff the minds of biologists need no longer be closed to the possibility of thinking out some other alternative.

It is ridiculous to go on talking as though energy were a stuff. And it will be desirable, in view of the prevailing attitude of biologists towards this notion of energy, to call attention to some statements of recent mathematico-physical writers on the question. Thus Mr. Bertrand Russell writes:

> 'Energy is a certain function of a physical system but is not a thing or a substance persisting throughout the changes of the system.'[2]

And Professor Whitehead:

> 'The doctrine of energy has to do with the notion of quantitative permanence underlying change.'
>
> 'Energy is merely the name for the quantitative aspect of a structure of happenings.'[3]

Thus energy is no longer thought of as an 'indestructible' metaphysical stuff or substance but gives expression to the recognition of a permanence in the quantitative characteriza-

[1] See the symposium: *Materialism in the Light of Modern Scientific Thought*. Arist. Soc., Supplementary Vol. VIII., 1928, p. 99.

[2] Herbert Spencer Lecture, Oxford, 1914, p. 10.

[3] *Science and the Modern World*, pp. 110-8.

tion of events. Similarly the doctrine of the 'conservation of energy' is an empirical generalization and not a 'principle' which can be used a priori to say in advance what must or must not take place, although this is the way in which, in accordance with tradition, it is apt to be employed in biological speculation. But, as Dr. Broad expresses it, 'even in purely physical systems, the Conservation of Energy does not explain what changes will happen or when they will happen. It merely imposes a very general limiting condition on the changes that are possible'.[1]

These changes in the scientific attitude towards energy have often been pointed out but the persistence of the old way of thinking testifies to the tenacity of the notion of stuff, the lack of sufficient communication between the sciences, and to the essentially pragmatic attitude of most biologists and their reluctance to indulge in anything in the nature of an 'unusually obstinate effort to think clearly'. But you cannot have it both ways: you cannot claim to be merely methodological in one breath and to be uttering metaphysical verities in the next on the basis of the same methodological postulates.

Group VI. We come now to the last and most difficult group of demands—those, namely, which cluster round the notion of causation. This group is of great importance in connexion with the antitheses to be discussed in Part II. We shall find examples there of difficulties created by an uncritical use of the notions of cause and effect. The chief difficulty in discussing in a general way the various notions comprised under the present group lies in their extreme vagueness and generality. It is exceedingly difficult to know what precisely is meant by such expressions as 'every effect has a cause'; 'the effect necessarily follows the cause'; 'the course of nature continues ever the same'; 'nature repeats itself'; 'the future will resemble the past', etc. It is easy enough to see vaguely what is meant but the difficulty is to be precise. What, for example, are we to understand by the following:

> 'Everyone will admit with Loeb that, given the same stimulus and the same organism, the reaction will be the same.'[2]

[1] *The Mind and its place in Nature*, p. 108.

[2] J. B. Watson, *Behaviour: an Introduction to Comparative Psychology*, New York, 1914, p. 107.

How can we test or make use of such a proposition? If I stick a pin into one of my fellow creatures and observe the 'reaction' and then, after an interval, apply the same pin to the same part of his skin it is extremely unlikely that I shall observe the same reaction. If the reaction is not the same the defender of the above proposition will exclaim that this is because the organism is not the same. He will say that the first application of the pin has modified the organism so that it is no longer the same organism and hence does not respond in the same way to the same stimulus. If, on the other hand, the reaction to the second application of the pin is the same as the first, then who ever wants to defend the above proposition can say that the first prick has not modified the organism, hence the latter is the same organism and consequently the reaction is the same. Thus *whatever* happens the proposition is saved. Consequently it is useless and does not really enter into the argument at all. A special difficulty arises in connexion with the expression 'the same organism'. An organism, whatever else it is, is an event—something happening. It is temporally as well as spatially extended. It has temporal as well as spatial parts.[1] Your pet dog to-day and your pet dog yesterday are two *different* temporal parts of the same dog, just as his head and his tail are two different *spatial* parts of the same dog. It is in virtue of the particular kind of continuity of the dog yesterday and the dog to-day that we call it the 'same', and this seems to be the proper sense of the term. But it can no more be taken for granted that to-day's temporal part is the same as yesterday's than it can be taken for granted that one spatial part, e.g. the head, is the same as another, e.g. the tail. We know, in fact, that they are not the same. Organisms are temporally as well as spatially differentiated and in this respect differ from lumps of iron. Accordingly even if we are 'given the same stimulus' we are never 'given the same organism' and such propositions as that quoted above appear to be meaningless. This does not mean that we are eternally condemned to talk nonsense about organisms but only that the avoidance of nonsense requires more care than is commonly supposed.

In Chapter III I maintained that 'remaining unchanged' or 'existing in a state' on the one hand, and 'exhibiting a causal law' or 'manifesting a property' on the other hand

[1] See below, pp. 299 and 301.

were both to be regarded as examples of recognizable permanences in nature differing from one another in the absence of change in the former and its presence in the latter. It is the business of natural science to discover such permanences. The demands which constitute this sixth group are supposed to be necessary assumptions for the prosecution of scientific research. Now in so far as such permanences have been discovered they are not assumptions. The question of assumptions only enters when it is asserted that they will continue indefinitely into the future, or that they have continued indefinitely in the past—the assumption, in other words, that the laws of nature have always been what they are now, and will always continue to be what they are now. This is the assumption which we ordinarily make in inductive inference, and it is this assumption which will arise later in connexion with certain biological problems.

It may reasonably be doubted whether any of the vague a priori generalities which are sometimes supposed to be necessary assumptions in science do in fact play any part in its actual procedure. Mr. Bertrand Russell writes:

> 'The principle "same cause, same effect," which philosophers imagine to be vital to science, is therefore utterly otiose. As soon as the antecedents have been given sufficiently fully to enable the consequent to be calculated with some exactitude, the antecedents have become so complicated that it is very unlikely they will ever recur. Hence, if this were the principle involved, science would remain utterly sterile.'[1]

It does not seem necessary to examine in detail in this place any of the various ways of stating the demands and postulates involving causation in general terms. Only when we come to actual biological problems which seem to presuppose them will it be necessary to turn to such questions again. But I should like to state here what I shall call 'the causal postulate' in a form which does appear to be important, and which is certainly used as a guiding postulate in biological investigation and explanation, in which so-called empirical generalizations are so much more important than in physics. This postulate may be stated as follows: If a change occurs in a natural entity this change is traceable (at least theoretically) to either (1) a change which has previously been occurring

[1] See the chapter on The Notion of Cause in *Mysticism and Logic*, p. 189.

in that entity although not previously observed or observable, or (2) to some change in other entities related to it, and upon which the change in it is dependent. In other words we have to do with a case either of immanent or transeunt causation.

The phrase 'traceable to' here indicates that it is believed that the change in question would not have occurred were it not that some other change had previously occurred. Now a given change of this kind is unique and therefore could not be investigated. If we are to make use of such a postulate the change in question must be such that it is either (1) repeated in the same natural entity; or (2) repeated in another entity belonging to the same natural kind all the members of which differ only numerically from one another. The first alternative presupposes that the change observed does not persist, but that the entity in question may return to the condition in which it was previously to manifesting this change. If it were a case of transeunt causation such a condition would be realized in the case of a dent made in a perfectly elastic body. Also in the case of immanent change of the rhythmical type the original state would recur, but in the case of immanent change of the serial type it would not. How far are these requirements realized in the case of a living organism during the developmental period? Clearly both are ruled out. For such an entity appears to exhibit change of the serial immanent type and consequently a given developmental change is not repeatable in the same embryo. Again, no two organisms belonging to the same natural kind are only numerically different, except (at least theoretically) in the case of 'identical' twins. Moreover, there are mnemic phenomena to be remembered, and these, according to the usual interpretation, require that a given stimulus should leave a *permanent* (or at least for some considerable time persistent) change in a given organism. It remains to be seen, therefore, how the causal postulate is employed in regard to genetical and embryological problems. This postulate will be of importance in Chapter IX when we try to understand and state clearly what exactly is involved in the topics there to be discussed.

Subjective Factors

Something must now be said about certain limiting and obstructing factors which were hinted at in the Introduction as apt to influence scientific theorizing and which are the concern primarily of the psychologist rather than the epistemologist. They concern the epistemologist only in so far as they become involved in the intellectual activity and give a bias to its outcome, but they are important in proportion as they are ubiquitous, deep-seated, unconscious in their operation and hence unacknowledged.

We will consider first those which are conservative in their tendency—keeping thought to one groove. In a recent paper on anthropoid behaviour it is said that:

> 'Evidence of fatigue in the apes, *especially in experimental situations which require novel adaptations* of behaviour, have been noted by various observers. . . . Bouton states that mental work rapidly fatigues the gibbon and induces yawning and finally sleep, and Yerkes has made similar observations on the chimpanzee and other primates.'[1]

Most of us will have made similar observations on ourselves if not on 'other primates' and the evolutionary biologist will have no difficulty in connecting the above remarks with that of Mr. Russell to the effect that 'Most people would sooner die than think—in fact they do so'. In other words fighting, being a more primitive function, is more popularly resorted to for the overcoming of difficulties than thinking. Even in purely intellectual—or supposedly purely intellectual—matters it is much less irksome to sit down and read what someone else has got to say, than to shut all books, decline the help of traditional views, and think out a problem with the aid only of one's own resources. Simian laziness, then, is one factor which makes for conservatism. Another aspect of this personal conservative tendency is well expressed by William James in the following passage:

> 'Nothing is more congenial from babyhood to the end of life than to be able to assimilate the new to the old, to meet each threatening violator or burster of our well-known terms of concepts, as it comes in, see through its unwontedness and ticket it

[1] R. M. Yerkes and M. S. Child, "Anthropoid Behaviour," *Quart. Review of Biology*, Vol. II., p. 44, 1927. (Italics not in text).

> off as an old friend in disguise. . . . We feel neither curiosity nor wonder concerning things so far beyond us that we have no concepts to refer them to or standards by which to measure them.'[1]

Dr. E. Rádl relates how C. F. Wolff, who overthrew the doctrine of preformation, was also one of the first to observe cells in plants, but instead of regarding them as something new he interpreted them as resulting from the expansion of fluids after the manner of the expansion of gas enclosed in dough.[2] To these individual conservative factors we have also to add those resulting from society—the so-called 'herd instincts' which in various subtle ways help to keep thought to its wonted channels. Is not the history of science filled with harrowing stories of the struggles of new ideas in the teeth of the opposition of contemporary traditional opinion? Another potent obstacle to change is success. The success of the simple notions of traditional mechanics is astounding. They have satisfied the requirements of physics for three centuries—no wonder they were regarded as keys to the very innermost secrets of nature—and so long as intellectual tools continue to receive fresh uses there is no stimulus to seek for new ones. Thus the four factors mentioned—natural laziness or simian reluctance, fondness of assimilating the new to the old, tradition and success—have contributed to keep our thought undeveloped. The periods of really intense intellectual ferment and tradition-shattering thinking have been extraordinarily few within the historical period. The thinking of Plato and Aristotle sufficed from Greek times to the Renaissance,[3] and the thinking of Galileo and Descartes at the Renaissance has furnished natural science with a stock of fundamental notions that have needed little revision until recent times. Thus during most of the intervening times thinking has chiefly been a process of working out of the thoughts which issued from those two periods of successful overcoming of the limitations from which the primate intellect naturally suffers.

Let us leave the subjective factors whose tendency is conservative and turn to a brief consideration of those whose

[1] *Principles of Psychology*, II., 110.

[2] *Geschichte d. Biolog. Theorien*, Teil II., p. 64, Leipzig, 1909. What precisely Wolff meant by this I do not profess to know. I merely mention this as an instance of the point under discussion.

[3] It is not suggested that the legacy from these two thinkers is even yet exhausted in all directions.

outcome is one-sidedness, dogmatism, intolerance and intellectual myopia generally. Most potent among these is 'selection' in various forms. Selection, wrote James, is the keel upon which the ship of knowledge is built. Continuing the metaphor we might add that it has sometimes been the rock upon which the ship has split. By selective subjective factors I mean those from which it results that our knowledge is piecemeal and fragmentary, but which, because their presence is ubiquitous and unnoticed, lead to our mistaking our little individual fragments for the whole, or for larger or more important fragments than they in fact are. The sense organs are selective. Interest, depending on deep-seated mental dispositions, is selective. The bare psychological impossibility of keeping in mind more than a limited number of aspects of a topic at once is selective in its consequences. The more we consider it the more we shall find reason to be on our guard against such selective factors. But their presence need give no occasion for despair, scepticism or anti-intellectualism. This is clear from the fact that it is possible to discover and overcome them, and of this fact Einstein offers a brilliant and exceptionally impressive example. But in this section I will confine myself to pointing out some features of what I have referred to above as 'deep-seated mental dispositions' particularly where affective or emotional elements are involved. Any one who is acquainted with the developments of morbid psychology during the present century will understand what I mean. It is plain from these researches that man is not such a 'rational animal' as the older psychologists who confined themselves to the cognitive processes supposed. It is a familiar enough fact of daily experience that more or less clearly marked types of 'temperament' are recognizable. William James attempted to interpret the well-known antitheses in the history of philosophical thought in terms of two contrasted types of temperament which he called the tender and the tough-minded. The tender-minded man inclines (according to James) towards rationalism, idealism, optimism and religion; whereas the tough-minded tends towards empiricism, materialism, pessimism, and irreligion. It is perfectly obvious that the cleavage is never so simple and clear-cut as this, but at the same time one cannot fail to be impressed with the way in which certain attitudes towards philosophical problems go together in one man and

a contrasting and often antagonistic set will be represented in another. It seems equally plain that these contrasting attitudes are not determined simply by intellectual or logical considerations. In constructing a monistic system of metaphysics such considerations alone can hardly determine which element in all our variegated experience is to be singled out as of supreme importance to which all else is to be subordinated. It will be subjective temperamental factors which will chiefly be responsible for this first selection and so will give a bias to the whole of the system.

Jung has elaborated James's view and extended it to other spheres—history, literature, art, etc., and so built up an ambitious theory of two main psychological types—the introvert type exemplified by James's tender-minded, and the extravert type exemplified in the tough-minded man. Roughly speaking the extravert is the man whose mental life is occupied with the 'objective' world of things—the practical, the outward-looking man, the man of action, impatient of theory. Whereas the introvert tends to be more concerned with the things of the mind and in general contrasts with the extravert. I am not at all concerned with the merits or demerits of this theory, nor does this brief reference profess to represent it at all adequately. I refer to it simply because it seems to me to throw light on our celebrated antithesis between mechanism and vitalism. It seems plain that this antithesis is itself in part at least an expression of the contrast between these two types. Most vitalists are much more philosophically minded than mechanists, and most forms of vitalism tend to take the form of postulating an active agent in the organism which is conceived on the analogy of the ego. Mechanists, on the other hand, usually exemplify the tough-minded or extravert type. They are less interested in theoretical questions and more interested in research itself. This is well illustrated by the remark of Loeb already quoted that 'Science is not the field of definitions, but of prediction and control'. Not for a moment do I wish to suggest that the problem is so simple as this brief reference suggests. Plenty of mechanists are clearly introverts and we usually find in such cases that they only hold to the mechanistic position on methodological grounds. This will be more clearly understood after the discussions of Part II. But nevertheless, in spite of complications and modifications in special cases, the theory

does appear to me to contain much more than a grain of truth. Neither do I wish to suggest that because this antithesis involves subjective factors of such a deep-seated nature it is therefore one which it is impossible to overcome. I am quite prepared to believe that if a man can be brought to understand that such subjective factors are operative in scientific thinking he will do his utmost to overcome them, but from the nature of his training it may be very difficult to show him this, and those same subjective factors will be operative in increasing the difficulty. Those who have been brought up in the ways of thinking proper to natural science appear to find great difficulty in getting a foothold in psychological literature, but all I can do here is to refer them to Jung's book for further information about his theory of types.[1]

It is a sorrowful reflexion to find how the free play of the intellect in its search for truth is obstructed and hedged round by so many opposing factors, and although we are encouraged by the success of the persistent effort to overcome them we cannot but feel impatient at its slowness. Many of these subjective factors were pointed out with great clearness by Bacon in the early aphorisms of the *Novum Organum*, particularly in those dealing with the *Idola*, but the biological sciences at the present day are full of illustrations of the fact that those same idola still find worshippers. And this seems inevitable in the absence of any counterpoise to specialism. For to specialize is to select, and to select is to attend to one thing and forget or neglect another, and so to regard it, for the moment at least, as unimportant. But what one man finds unimportant another finds important, and so we have the conditions established for an 'antithesis' and perpetual misunderstanding and needless controversy. It is *needless* controversy grounded on ignorance springing from limitation of outlook and neglect of methodological fundamentals which is so deadly and unfruitful. A ray of hope offers itself in the suggestion that students should be given opportunities of coaching and discussion on scientific methodology before they plunge into special researches. But there are strong conservative tendencies opposed to this reform.

Occasions have already arisen for calling attention to certain misunderstandings regarding the process of abstraction. Because this is an essential and inescapable feature of all

[1] *Psychological Types*, Eng. trans., London, 1923.

cognitive activity it is not to be supposed that it is infallible, neither need it be concluded that because it too is 'selective' it is necessarily fallacious. If a method of abstraction is successful in a certain sphere I take this to mean that certain genuine pervasive features of that sphere have been successfully discriminated, and propositions about that sphere on the basis of such a scheme of abstraction will have a high degree of probability within those limits. But two restrictions will have to be remembered. First, that the success of such a system of abstraction need not exclude all possibility of exploring others; and secondly there will be no reason to suppose that such a scheme can be applied a priori to a different sphere, still less that it will be exhaustive in that sphere, since no scheme of abstraction can be exhaustive, otherwise it would not be abstract.

It is well to bear in mind that natural science has so far only explored part of reality with great thoroughness, and that part only after abstraction has been made from it. Moreover the intellectual weapons it has used were tools forged originally for the very limited purposes and range of everyday life. It need not, therefore, surprise us in the least if what remains does not fit into the prevailing scheme of thought. Neither need it surprise us to find that a mode of abstraction devised long ago as a methodological postulate at the dawn of physical science should lead to metaphysical consequences of a kind to impress themselves so strongly on the mind as to create difficulties for biological thinking, which has to deal with problems not at that time contemplated. Galileo was not thinking about 'heredity' or evolution, but about pendulums, balls rolling down inclined planes and the like. In so far as the pervasive features of the world which Galileo discerned extend from such processes as these to biological processes, just so far will his scheme of abstraction, which gave expression to such features, also be applicable to biological problems. But to use such a scheme intolerantly, to expect it to be exhaustive, and to discourage all attempts to explore other possibilities is to put metaphysics before science, dogmatic presuppositions before unbiassed observation, and an impatient desire to erect the superstructure before prudent attention to the foundations.

Observation—the continual piling up of data—is not enough, as Professor Whitehead points out, to correct the

effects of adhering to one mode of abstraction, since observation itself is selection. 'Accordingly it is difficult to transcend a scheme of abstraction whose success is sufficiently wide. The other method is by comparing the various schemes of abstraction which are well founded in our various types of experience.' And this means that 'reason should be used'.[1]

* * * * * * * *

I will conclude this chapter by giving a bare list of fundamental types of judgments upon which all our natural scientific knowledge rests, and which cannot be inferred from anything else:

A. Empirical.

Judgments of Perception, e.g. "This rabbit is white".

B. A Priori.

(A priori not in the sense of being known prior to experience but in the sense of being logically independent of experience: 'elicited by' experience, but neither provable nor disprovable by experience).[2]

1. Not "self-evident" but essential for the possibility of natural scientific knowledge:
   a. Belief in a real external world.
   b. Belief in the trustworthiness of memory.
   c. Belief in the existence of other selves.
   d. Belief in some sort of regularity in the external world.

2. Self-evident as well as essential for knowledge.
   a. Analytic (in the sense that the opposite would be self-contradictory).
      e.g. 1 foot equals 12 inches.
      (i.e. defining and explicatory judgments).
   b. Synthetic. e.g. propositions concerning the relations between universals—'black is different from white' ['das Sosein Schwarz und das Sosein Weiss sind verschieden'][3] 'The whole is greater than the part.' 'If a equals b and b equals c, then a equals c' 'If a entails b and a is true, then b is true' (Where a and b are propositions, and 'a entails b' means 'b logically follows from a'.].

[1] *Science and the Modern World*, Cambridge, 1927, p. 23.
[2] Cf. B. Russell, *Problems of Philosophy*, London, 1910, pp. 109-141.
[3] E. Becher, *Geisteswissenschaften und Naturwissenschaften*, München und Leipzig, 1921, p. 44.

PART II

PROBLEMS OF BIOLOGICAL KNOWLEDGE

CHAPTER V

The Antithesis between Vitalism and Mechanism

The celebrated quarrel between vitalism and mechanism, which is such a striking feature of the history of biological science, is by no means such a simple one as is sometimes supposed. The issues involved and the various opinions represented are by no means simple and clear cut. Consequently our first duty is to disentangle these issues a little before we pass on to examine some representative examples. There is considerable and important divergence of opinion among those who are called (even if they do not call themselves) 'vitalists', and there are no less important differences of opinion among those who belong to the opposite camp and accept the title of 'mechanist'. Consequently, to say that so-and-so is a mechanist is to convey very little information until his particular brand of mechanism is also stated. It will be helpful, in what is to come, to point out at the beginning the characteristics of the main cleavages. What may be called the 'mechanistic view' of the organism may base itself on one of two main grounds: (1) It may be believed that the organism *is* in some sense a machine, or a 'mechanism' and nothing more. This we may call a metaphysical, in the sense of ontological, basis. (2) On the other hand we may avoid making any metaphysical or ontological assertions about the organism, and simply say that, whatever the 'nature' of the organism may be, we can only deal with it scientifically by treating it *as if* it were a machine or a 'mechanism'. Thus

we have two fundamentally different kinds of mechanism. The first is dogmatic and metaphysical in the sense explained because it professes to say that the organism *is* a machine, whereas the second makes the more modest claim that science is only possible if it adheres to mechanistic explanations, but it abstains from making any statement about the ultimate metaphysical nature of the objects of biological study. This, then, is a *methodological* basis.

The primary cleavages among the vitalistic doctrines are not quite of the same nature. But there is one group to which the title vitalism is applicable in the strict sense: these all assert that in addition to the organism as studied by their rivals there is another entity of a totally different nature the existence of which is revealed by the peculiarities of organisms. This has been the characteristic of all the historical vitalisms and might be called dogmatic or metaphysical vitalism since it makes a positive ontological assertion about the nature of organisms. The remaining writers on this topic agree with the vitalists *sensu stricto* in opposing the mechanists of both denominations, but differ from the vitalists in their positive assertions. What those positive assertions are is not easy to say in a few words. It will be better at this stage simply to call this group 'antimechanists' and so to avoid confusing them with vitalists as is commonly done. Let us return to the two chief varieties of mechanism.

Everyone is agreed that from the heuristic standpoint the mechanistic view has been successful, although no one claims that it has yet reached its goal. Dogmatic mechanists, who believe that the organism *is* a machine, contend that this is only because sufficient time has not yet elapsed. Dogmatic vitalists, on the other hand, since they believe that the organism is in no sense wholly a machine, claim that the mechanistic goal can never be reached. Thus both parties venture on prophesy and in so doing each makes a claim which only the future can settle. Both parties have made up their minds and decided the problem in advance, and there is no possibility of reconciliation between them. *One* of them is certainly wrong and both *may* be wrong. Each party will, of course, profess to base its claim on experience. The mechanist will be able to point to a long series of triumphs in the past and to the short period during which active research on his lines has been pursued. Being a firm believer in the 'uniformity

of nature' his final success will seem to him to be a foregone conclusion. He will wonder how any reasonable man can possibly fail to share his opinions and will conclude that his opponents cannot be reasonable men. He will accuse them of being the victims of prejudice and other 'subjective factors' never dreaming that he may also be a victim of them himself. The dogmatic vitalist, on the other hand, will contend that living things, since they are not yet explicable in mechanistic terms, and since they exhibit peculiarities which are not encountered in the inorganic world, belong to a different order of being. His faith is not shaken by his opponent's success because he has long and complicated arguments which (in his opinion) place those successes in their proper perspective. But his opponent's faith is equally unshaken by such replies —chiefly because he does not read or understand them. Thus the vitalist concludes that his opponent is a man of crude sensibilities and inferior intellect, and the dead-lock is complete. The differences between these two views rest on differences of *opinion* about the correct interpretation of what is given to us in experience. If we have already made up our minds about the general form which that interpretation is to take we have settled the so-called 'problem of life' and there is no profit in further discussion. The position is a stalemate and we can only wait and see which prophecy comes true.

We can turn, then, to the methodological standpoint and ask the methodological mechanist what he has to say in support of his contention that the mechanical explanation is the only one which is admissible in science. This is clearly a contention of quite a different kind from the first. The dispute now is about *explanations* not about organisms. It is therefore not a biological problem at all but a logical one (in the wide sense of the term). Confusion may result from the failure to recognize this change. Another source of confusion lies in the differences between the investigator and the theorizer. The former will be content with a scheme which brings heuristic success, and the mechanistic view appears to have the prior claim in this field. Consequently those who profess methodological mechanism may do so simply from this standpoint. But from the standpoint of theoretical biology the position cannot be left in such an unsatisfactory state, and the problem is much more difficult than it seems. The biological methodologist is confronted with the difficult task

of trying to determine not whether the mechanistic view is successful but whether it is true that this is the only possible one for scientific biology. If this is true the consequences are obvious. If it is not true then other possibilities will have to be explored. Further difficulties are created, as we shall see later, by the fact that the expression 'mechanical explanation' is extremely vague, and this fact alone is responsible for a great deal of confusion.

Sometimes it is said that the mechanistic view has 'kept us moving in the right direction'. (E. B. Wilson.)[1] This implies that you know where you are going to and which is the right way there. A compass will keep you in the right direction provided you know which direction you want to keep to, but a mariner who set out only with a compass, and a chart which was a 'perfect and absolute blank', would have a voyage not unlike that described in *The Hunting of the Snark*. A mechanistic point of view must, it would seem, lead only to one result. If our system of demands is such as to exclude any but mechanistic notions we can only expect a mechanistic outcome to thinking. Thus if methodological mechanism proceeds in this way it also may be said to have solved the problem in advance of particular investigation, in so far as it closes the door to other possibilities. Only from the heuristic point of view can we say with certainty that mechanism has kept us moving in the right direction. But clearly this is of no help from the theoretical standpoint, because a heap of data does not make a science.

It is, of course, open to any *branch* of biology to define its task as the investigation of the physics and chemistry of the organism. This is very commonly done in physiology, even although the programme is not consistently carried out. But we are concerned here not with a branch but with the whole, with the question namely, whether it is possible to represent the mechanical explanation as the only *possible* one on *purely* methodological grounds. To do this it is obviously necessary to show that the modes of thinking of the human mind which are available for scientific explanation are limited to those employed in the mechanical view. It is hardly surprising to find that this is not done by investigators who hold methodological mechanism. They are too busy to attend to such matters, and are convinced by their scientific

[1] See below, p. 256.

'instincts' or 'intuitions' that the methods which they have found to be heuristically successful are sufficient. To show that methodological mechanism is the only possible scientific position would require first a thorough logical investigation of the nature of mechanistic explanations and secondly a demonstration either (1) that no other way of thinking is possible, or (2) that no other way can possibly be called scientific. If the latter course is taken we can easily fall into a mere logomachy about the use of the word 'scientific'. Those who take this course usually make a sharp distinction between what they call scientific thinking on the one hand, and philosophical thinking on the other.[1] It is here that we find the introverts who are not vitalists. They *identify by definition* scientific thinking with the mechanistic view, and philosophy, as they understand it, appears to be given the task of dealing with the problems or aspects of the organism which escape the meshes of mechanism. Those who advocate this position in its extreme form appear to identify 'science' with mathematical physics—everything which fails to come within its scope being excluded. Thus the concept 'organism' is regarded as extra-scientific. So far so good. But when this view is taken seriously it is very difficult to see what is to be done with the vast bulk of what passes under the name of science, since it would certainly be repudiated by philosophy as falling outside its province. The amount of biological knowledge that is amenable to treatment by mathematical physics at present is extremely small. But pending such absorption it is a legitimate field for inquiry and the people who undertake such inquiry are not philosophers. The only consistent thing to do on the above view would be to turn out from their chairs all existing professors of biological sciences who do not pursue their studies in accordance with the principles of systematization employed in mathematical physics, and to give all students whose aim is to become biologists a thorough training in mathematics—omitting all such extra-scientific concepts as 'cell', 'inhibition', 'evolution', 'organ', etc., from the scientific vocabulary.

It is possible that the above view owes its origin very largely to a misunderstanding of a remark of Kant in his *Metaphysische Anfangsgründe der Naturwissenschaften*. This celebrated remark is frequently misquoted and still more often

[1] See above, p. 208.

misinterpreted. Thus Professor J. A. Thomson, although he does not subscribe to the above view, says: 'It was Kant who said that any branch of knowledge contains just so much *science* as it contains mathematics.'[1]

Now what Kant actually wrote was as follows:

> 'Ich behaupte aber, dass in jeder besonderen Naturlehre nur so viel eigentliche Wissenschaft angetroffen werden könne, als darin Mathematik anzutreffen ist.'[2]

But unless the reader knows what has gone before, this passage, as it stands, is extremely misleading. Its interpretation depends entirely on the special meaning which Kant gives to the expression 'eigentliche Wissenschaft' at the beginning of the treatise. In the Preface he begins by saying that every doctrine is called science if it is a system, i.e. a whole of knowledge systematized according to principles. Next he says that such doctrines (Lehre) can be divided into 'historische Naturlehre' and 'Naturwissenschaft'. The first contains nothing but systematically arranged facts about natural things—either classifications according to similarities (Naturbeschreibung) or a systematic representation of them according to their distribution in time and space (Naturgeschichte). The second division (Naturwissenschaft) he then divides into 'eigentlich, oder uneigentlich so genannte Naturwissenschaft'. The first (eigentlich) deals with its object solely according to a priori principles, and the second according to empirical laws. Kant then goes on to say that only science whose certainty is apodeictic can be called 'eigentliche Wissenschaft'. Now at the present day it is no longer believed that any *natural* science can claim apodeictic certainty, and consequently *no* natural science is 'eigentlich' in Kant's sense.[3] The only science in which we can speak of apodeictic certainty is formal logic in its present development known as 'logistic' or 'symbolic logic' and its special extension commonly called pure mathematics. But this is not natural science, and if, when we speak of 'exact science', we mean exact in the sense in which mathematics is exact then physics is not an exact science. Exactness in the mathe-

[1] *Introduction to Science*, London, p. 53.

[2] *Kant's Gesammelte Schriften*, herausg. v. d. K. Preuss Acad. d. Wiss. Bd IV., p. 470.

[3] Even here we only have to do with *logical* certainty, in the sense in which one proposition *follows* from others with *logical* necessity.

matical sense means precision of definition and stringency of logical deduction, in other words exactness of *thought*. Exactness in the physical sense means exactness of *measurement*, and this usually means closeness of *approximation* to an ideal mathematical law, which is quite a different thing from exactness in the mathematical sense. Some departments of biology may attain to exactness in the second of these senses but it is very far from exact in the first. It is hampered by the lack of clear ideas because its votaries have not yet learnt the necessity for such. But in the absence of clear ideas no amount of measurement or mathematics will avail. In this connexion the following passage from Whitehead, whose books are full of methodological wisdom, is of special interest:

> 'Mere deductive logic, whether you clothe it in mathematical symbols and phraseology, or whether you enlarge its scope into a more general symbolic technique, can never take the place of clear relevant initial concepts of the meaning of your symbols, and among symbols I include words. If you are dealing with nature, your meanings must directly relate to the immediate facts of observation. We have to analyse first the most general characteristics of things observed and then the more casual contingent occurrences. There can be no true physical science which looks first to mathematics for the provision of a conceptual model. Such a procedure is to repeat the errors of the logicians of the middle ages.''[1]

Substitute the word 'biological' for 'physical' in the above passage and I think it will apply equally well to our problems. Given some theory expressed in clear concepts and deductive logic will enable you to work out its consequences and you can appeal to experience to see whether they are upheld, but your theory must in the first instance be based on the 'general characteristics of things observed', which for biology means living organisms. No natural sciences can be said to *contain* mathematics, they only *use* it as a device to save mental labour.[2] The real thinking is done in getting the data into a shape which will admit of such treatment. Consequently Kant's saying is of little importance for our present problem, and it cannot be appealed to to justify the restriction of the term science to mathematical physics. Physics is scientific not because it admits of mathematical treatment but because it possesses 'clear relevant initial concepts'. There is doubtless plenty of scope for the use of mathematics in biology

[1] *The Principle of Relativity*, p. 39. Also *Intro. to Mathematics*, p. 31.
[2] Cf. Whitehead, *Intro. to Mathematics*, p. 61.

if the right way to set about it is discovered, but it does not follow that the right concepts for this purpose are those at present used in physics.

It does not seem, then, that a theoretical justification for methodological mechanism is quite so obvious as it is sometimes taken to be. Moreover a doctrine of the organism may be mechanistic in one sense without involving mathematics at all. Consequently we cannot begin to consider whether it is the only possible mode of thinking in science until we have unravelled further these various uses of the term 'mechanistic'. We shall also find that the situation is made still more complicated and confused by divergence of opinion about what the precise aim of science is. This was touched upon in the Introduction. Those who believe that the aim of science is simply prediction for utilitarian purposes will naturally be perfectly satisfied with any scheme which fulfils this requirement, and some such scheme as is offered by mathematical physics will appear to them to be their chief goal. From this standpoint science becomes a kind of game in which the rules are laid down by mathematical physics. Everything which does not conform to the rules is regarded either as simply awaiting further analysis, or as being extra-scientific, and therefore to be handed over to philosophy, which is another game with different rules in which people amuse themselves with the 'unscientific half' of existence.

The Mechanical Explanation

A great deal of the misunderstanding and wrangling that goes on about mechanical explanation in biology could be avoided if a little more attention were first paid to deciding what exactly we mean by this expression. Dr. C. D. Broad, who has contributed a useful discussion of this question, remarks that:

> "... the combatants all assume that everyone is agreed as to what is meant by a mechanical explanation, and, presumably in consequence of this assumption, never condescend to inform the reader what they in particular mean by it. I strongly suspect that this belief in a general agreement indicates nothing but a general haziness."[1]

[1] *The Mechanical Explanation and its Alternatives*: Proc. Arist. Soc. 1919, Vol. XIX., p. 86.

This suspicion is amply fulfilled by a survey of the various expressions of opinion on the subject by biologists. Every writer appears to have his own private view about what is meant, but as he never states it clearly and concisely it can only be surmised and pieced together from his assertions about its applications to biology.

The best authorities to go to for information on this question would appear to be those who employ the mechanical explanation in the sphere for which it was devised, namely mathematical physicists. But this precaution too seems to be neglected by biologists. Their physics is too often that of the elementary text-book which is not only very dogmatic but represents the state of the science in the Victorian era. But the physics of to-day is a very different affair from that of the great Victorian physicists, and the mechanical explanation occupies a very different position from what one would suppose from the text-books. Nothing will illustrate this more clearly than the following passage from Professor P. W. Bridgman's *Logic of Modern Physics* (p. 46)—a book which every biologist could read with great advantage:

> 'It is difficult to conceive anything more scientifically bigoted than to postulate that all possible experience conforms to the same type as that with which we are already familiar, and therefore to demand that explanation use only elements familiar in everyday experience. Such an attitude bespeaks an unimaginativeness, a mental obtuseness and obstinacy, which might be expected to have exhausted their pragmatic justification at a lower plane of mental activity.
>
> 'Although it will probably be fairly easy to give intellectual assent to the strictures of the last paragraph, I believe many will discover in themselves a longing for mechanical explanation which has all the tenacity of original sin. The discovery of such a desire need not occasion any particular alarm, because it is easy to see how the demand for this sort of explanation has had its origin in the enormous preponderance of the mechanical in our experience. But nevertheless, just as the old monks struggled to subdue the flesh, so must the physicist struggle to subdue this sometimes nearly irresistible, but perfectly unjustifiable desire.'

This passage is of great interest in the present connexion and especially in view of the fact that it is addressed by a physicist to physicists without any reference to biological problems. But before we attempt to determine how the mechanical explanation stands at the present day I shall first examine some opinions of a number of well known biologists, and try to collect together the various views on the subject at present in vogue among them. It should also

be repeated at this point that we are not at all concerned with the question whether an organism is or is not a machine, or whether we have to postulate various 'agents' in order to 'explain' organisms. Such questions do not belong to the logic of biology, and it seems doubtful whether they can, strictly speaking, be discussed. All that one can do is to speculate about them and try to represent observed facts as favouring one side or the other. 'Metaphysics', wrote the late Mr. Bradley, 'is the finding of bad reasons for what we believe upon instinct.' Science, on the other hand, is concerned not with what people believe, but with what is the case, and in the present chapter we have to discuss the *methodological* question whether organisms are, must, or can be, treatable from the scientific standpoint *exclusively* as machines or mechanisms, and if so in what sense. We are concerned, that is to say, with the nature of the mechanical explanation, whether it is suitable for biological purposes, and if so to what extent. Even if organisms are in no sense machines it may be that scientific explanation is only possible in the biological sphere on the supposition that they are in some sense machines. If this could be shown to be the case a great deal of unnecessary controversy would be avoided.

We shall first consider the following expression of opinion by Max Verworn who writes that modern neovitalistic efforts

> 'originate solely in a widespread confusion regarding the boundaries of natural science, their principal tendency being to amalgamate psychological and speculative questions with problems of purely natural science. In the face of these efforts, which by their unfortunate designations of Vitalism and Neo-vitalism give rise to entirely false conceptions, and which by their intermingling of psychological questions and questions of natural science have led to mere confusion in research, it is essential that natural philosophy should be called upon to realise its own limits, and above all clearly to understand that the sole concern of physical science is the investigation of the phenomena of the material world. Physiology, as the doctrine of life, must therefore confine itself to the material vital phenomena of organisms. It is self-evident however, that only such laws as govern the material world will be found governing material vital phenomena—the laws i.e. . . . (of) mechanics. The explanatory principles of vital phenomena must therefore be identical with those of inorganic nature—that is, with the principles of mechanics.'[1]

[1] *Encyclopaedia Britannica*, 11th ed., vol. XXI., p. 554, art. Physi-

It is not difficult to understand and sympathize with the contention that it is undesirable to confuse one science with another, and most people would wish to avoid amalgamating psychological questions with problems of purely natural science. But the difficulty in the above passage is to discover what precisely is meant by natural science and how it comes to be suggested that it is free from 'speculative questions'. But first psychology is contrasted with natural *science*, then we are told that natural *philosophy* must realize its own limits, and then that *physical* science is solely concerned with the phenomena of the material world. No one would demur to the last proposition if by phenomena of the material world is meant the world of sense perception, but it is not clear from the above what the relation between natural science, natural philosophy and physical science is, unless they are to be regarded as synonymous. And how is it possible to jump from this with a 'must therefore' to the proposition that physiology as the science of life is to confine itself to the material phenomena of organisms? Verworn seems to suggest that there are non-material phenomena connected with life, but if this is the case then physiology on his view cannot be the 'science of life' but only of part, and there are moreover other branches of biology than physiology which have an equal claim to a place in the 'science of life'. But this is a small point compared with the astonishing statement that it is *self-evident* that only such laws as govern the material world will be found governing material vital phenomena. If we knew that the material world was a perfectly uniform homogeneous gas it might be tolerably safe to assume that all parts were governed by the same laws, but as this is not the case it is anything but *self-evident*. Neither is it true that the laws governing the material world are exclusively those of mechanics —at least not those of mechanics as the physicist understands the term. Consequently it is impossible to determine the 'explanatory principles' of vital phenomena by means of a 'must therefore'. There is no 'must' at all in natural science; 'must' belongs to formal logic. In any case it is untrue to say that physiology must only use the explanatory principles of mechanics because physiologists do in fact use a great many explanatory principles which are not to be found in mechanics.

This first witness, then, has not proved very helpful, but

it is difficult to understand Verworn's position when we recall what he says in his *Allgemeine Physiologie*, which we discussed in Part I (p. 100).

> 'the laws of the physical world are the laws according to which our own psychological phenomena occur, because the physical world is only our idea. All science, therefore, is in this sense psychology.'

I find it impossible to understand what this distinguished physiologist really does mean from all these contradictory statements.

The next example is from a paper by Professor F. H. A. Marshall entitled *The Categories of Biological Science*.[1] There is a great deal in this paper with which many people of both sides would agree, but it does not clearly distinguish the issues; the author misunderstands the views he criticizes and consequently the whole problem appears simpler to him than it really is. Moreover Professor Marshall does not say at the outset what exactly he means by 'categories' and this initial vagueness is responsible for infecting the whole discussion. Sometimes he simply means the general concepts of a science, and sometimes he uses the term 'category' in the Kantian sense. Professor Marshall begins by referring to those who answer the question 'whether the categories of biology are ultimately reducible to those of chemistry and physics' in the negative; and to those who affirm that 'biology is an autonomous science with the right to its own conceptions and terms which need not and cannot be replaced by those of the inorganic sciences'. By 'categories of biology' here he no doubt means simply such general biological concepts as organism, cell, evolution, etc., and the question clearly is whether such concepts are, need be, or can be reducible to or replaced by the concepts of the inorganic sciences. Professor Marshall next states that the aim of his paper is to discuss the question 'whether it is possible profitably to carry on biological research and to aim at making biological generalizations without perpetual reference to the methods and categories of the inorganic sciences'. Now this is by no means the same question as the first. We might easily answer it

[1] *Mind*, N.S., Vol. XXIX., 1920, p. 62.

in the affirmative and yet be in doubt about the correct answer to the first. For example Darwin and Mendel both profitably carried out biological research without perpetual reference to the physical sciences, in fact with very few such references at all. Of course the expression 'methods and categories of the inorganic sciences' is ambiguous. If it simply means using your eyes and fingers and the categories of substance and causation then the answer will perhaps be in the negative, but if it means using the methods of analysis, and the fundamental concepts, *peculiar* to the inorganic sciences then the answer certainly is in the affirmative. Mendel's work was purely biological both in method and concepts, unless counting and measuring is to be called physical in which case it was to that extent physical. But the theory as he stated it was in purely biological terms. Whether it will *ultimately* be possible to state it in physico-chemical terms is quite a different question. And yet it may be possible to regard it as in one sense mechanistic, and consequently a theory may be mechanistic without being stated in physico-chemical terms. The antithesis here is between biological and physico-chemical, not between biological and mechanistic. But unfortunately Professor Marshall in some parts of his paper, seems to suggest that 'biological' means 'teleological'. From the example given it is clear, however, that you can be scientific in biology without talking about physics, chemistry or teleology. Professor Marshall also devotes pages to showing that 'biological study is being advanced by the use of chemical and physical methods'. But surely no one wishes to deny this, and it does not in the least render unnecessary biological investigations which use purely biological notions—such, for example, as those employed in experimental embryology or neurology. Neither does the fact that physical and chemical notions are useful in biology settle the question with which Professor Marshall began. And the same seems to me to be true in regard to two quotations which Professor Marshall brings forward in support of his contentions. He quotes Professor Sir Edward Schafer as saying: 'The phenomena of life are investigated and can only be investigated, by the same methods as all other phenomena of matter.' Surely no one would disagree with this if by 'phenomena of matter' is meant that which we observe by the senses. But there is an immense variety among such methods—those of chemistry, of physics,

of geology, and of biology. And this decides nothing regarding the reducibility of the *concepts* of one science to those of another.

The second quotation is from Claude Bernard: '. . . all the phenomena which make their appearance in a living being obey the same laws as those outside it.' Now this, of course, is simply an hypothesis, not an a priori law which can be laid down as a dogma to decide anything in advance of inquiry. But it is a very important hypothesis. It implies that the relations between the parts in a living body are external relations, so that the fact that they *are* parts of a living body makes no difference to them. This is, as we shall see, the fundamental point at issue, but physiologists all unconsciously assume that the parts of an organism *are* externally related simply because they all (or nearly all) assume that what they are studying is a machine and in a machine the parts are usually regarded as externally related. But simply to assume that this is so and to forget that you have made an hypothesis is merely unconscious dogmatizing, and no progress with the present problem can be made in that way.

A further complication in Professor Marshall's paper is his declaration of the phenomenalist's faith. This has already been referred to in the Introduction and need not be gone into again here. He says that 'a law of nature is not a statement of fact but a policy. A sound policy having been adopted, the things we expect are the things that come about . . .' This being the case we can understand that the author is not interested in the problems of biology but simply in discovering a policy by which we can breed fatter cattle or avoid sickness, and seeing that physics has found a good policy we need not trouble ourselves with seeking for any other. If we adopt this position there is no more to be said.

But Professor Marshall does say more. He devotes a great part of his paper to the criticism of the opinions of Dr. J. S. Haldane. By examining these criticisms we shall therefore have an opportunity of introducing the views of Dr. Haldane and of trying to understand how he differs from other physiologists. To me it seems plain that Professor Marshall has completely failed to understand Dr. Haldane chiefly because he has a preconceived notion of what Dr. Haldane means and this prevents him from finding out what exactly the latter does mean. One thing especially which prevents Professor

Marshall from understanding that there can be any other point of view is his curious identification of 'biological' with 'teleological': he persists in treating these terms as synonyms. We can begin by considering the following difficulty put by Professor Marshall:

> 'What would it have profited if Prof. Loeb, in treating of fertilization, had started on the assumption that a physico-chemical theory was improper or irrelevant, and had, as an alternative, put forward some vague generalization, such that the sperm and ovum are actuated by an inherent purposiveness which induces them to adjust themselves to one another, thereby acquiring a new vitality, to the end that a new generation might be produced? This is hardly a caricature of the method which Dr. Haldane advocates, but which, to the great advantage of physiology, he does not seem to carry out in his work.'
>
> '. . . this same work seems to me to be a remarkable illustration of the ever-increasing adequacy of the physical and chemical categories in the interpretation of organic phenomena.'

But has Dr. Haldane really ever advocated anything quite so silly as this? Does it seem at all probable that a man of his acknowledged ability would propose such a thing? The possibility suggests itself that what Dr. Haldane does teach is a little too subtle for the average physiologist and that what the above is *not* a caricature of is Professor Marshall's own understanding of Dr. Haldane. I am not concerned here to defend Dr. Haldane's contentions but merely to try to understand them. But before we turn to this it will be well to point out that Professor Marshall's imaginary example could easily be turned against him. What would it have profited Mendel if, in treating of hybridization, he had started on the assumption that purely biological notions were irrelevant or improper, and had put forward some vague untestable supposition about the physico-chemical nature of the germ-cells? The fact that he proceeded in quite another way is surely a testimony to his wisdom.

But let us look now at some of the things Dr. Haldane has said, and it will be as well to point out at once that the key to the understanding of his position is in the denial of that hypothesis of Claude Bernard's. Professor Marshall himself quotes Dr. Haldane as saying: 'If we are to get a grip of biological fact—the grip which enables us to predict—we must always keep the whole organism in view.' And this is diametrically opposed to Claude Bernard's hypothesis that: 'all the phenomena which make their appearance in a living

being obey the same laws as those outside it'. But Professor Marshall does not discuss these two important alternatives but simply repeats that: 'there is nothing to be gained for biological research by seeking to interpret life in terms of ends or purposes',[1] and this, so far as I can discover, is not exactly what Dr. Haldane wishes to do. Thus as is usually the case in biological controversies, these eminent physiologists are talking about two totally different things. We can take as our source Dr. Haldane's *Mechanism Life and Personality*. On p. 5 of that book he writes:

> 'If we are investigating secretion we are measuring the mass or volume of the substances secreted, or their chemical composition, or perhaps their osmotic pressure, or concentrations of ions in them. If we are investigating muscular contraction we are measuring the rate and extent of the contraction, or the accompanying heat production or chemical or electro-motive phenomena. The phenomenon which we observe is always some physical or chemical change. The methods we use are physical and chemical methods, and the resulting facts are consequently physical and chemical facts.'

There is nothing here to arouse the disapproval of the most sensitive champion of biological orthodoxy, nor does it in the least contradict the details of Dr. Haldane's own procedure in the investigation of breathing. But notice that the author does not use the expression 'explaining' or 'interpreting' in the above passage, he speaks only of *investigating* and *measuring*, and of methods of so doing. Also he speaks of physical and chemical changes and facts. Let us see then what Dr. Haldane says when he comes to interpretation. Consider the following passage (p. 91) on 'defects of current physiology.':

> 'If . . . we are
> must teach the main facts [illegible]
> secretion of urine. We must discuss the possible
> filtration, diffusion, etc., in the process, leaving out of account all details which are irrelevant to this discussion, and when at the end it turns out that the essential mechanism of secretion is quite unknown there is nothing further to do than pass on to the next subject. Actually it is known that, mechanism or no mechanism, the kidney fulfils its functions of regulating the composition of the blood, and that it does so with marvellous delicacy; but facts relating to this do not fit into the plan of exposition of the subject, and have too much of a smack of old-fashioned teleology about them. Hence they are ignored completely, or scarcely touched upon.'

[1] For contrary opinions of other physiologists see below, p. 430.

> 'The fact that the body lives as a whole, each organ or part fulfilling its proper functions and adapting itself to every change, is scarcely touched upon, while a vast mass of unrelated and unassimilable mechanical detail is carefully recorded and described.'

Dr. Haldane's contention seems so far to be that in expounding physiology we must always bear in mind the whole organism of which the various 'mechanisms' into which it can be artificially analysed are parts, and we should expound what we can discover about those parts from the point of view of their significance in the whole. This seems to be a perfectly innocent and legitimate contention—especially when the needs of the medical student are remembered. But Professor Marshall does not discuss this contention of Dr. Haldane's because, apparently, he thinks that Dr. Haldane means something else. And of course Dr. Haldane does mean something more and it remains for us to try and find out what that something is. The fundamental divergence between Dr. Haldane and orthodox physiologists seems to lie not in methods of investigation but in what he calls the 'fundamental conception or working hypothesis' which is habitually used 'in dealing with the phenomena of life'. Dr. Haldane contends that 'In dealing with life we not only use a whole series of special terms, but these terms appear to belong to a specific general conception which is never made use of in the physical sciences.'[1] And if we read carefully what Dr. Haldane says in the pages immediately following the last passage it will be clearly seen that he is contending that the notion of the organism as in some sense a unitary individual thing enters into all biological propositions. He also urges that a false separation between structure and function should not be made, and that the significance of organization should not be forgotten. Moreover as regards relation to the environment, what goes on in the metabolic exchange 'in so far as it has any physiological significance, is always determined in relation to the rest of the living activity of the organism'.[2] All this may well be of importance and there does not appear to be anything here suggested which is at all opposed to physiological progress. It may even be that physiologists do in fact unconsciously use such notions in their work, but because they are a trifle subtle and not obviously on the surface they may pass unnoticed. But Dr. Haldane goes further still. He explicitly

[1] *Op. cit.*, p. 77. [2] *Ibid.*, p. 79.

states that he rejects the metaphysical notion of matter entirely. He does not conceive the organism as consisting of little bits of stuff pushing each other about. In this respect he is in line both with philosophical criticism and with modern physics. Moreover he urges that 'apparent physical and chemical changes are the signs or sensuous data which point to the underlying activity'. The fundamental concept is not to be 'matter in motion' but an 'underlying activity' manifesting itself in individual unities. Thus what Dr. Haldane is asking physiologists to give up is *not their methods of investigation*—which are the same as he himself uses—but the intellectual background of naïve materialism. He is asking them, in the words of Professor Whitehead, 'to become philosophical and to enter into a thorough criticism of their own foundations'. He simply takes the epistemological investigations of the last three hundred years seriously and sees that they will not let you go on in the old way. And it is extremely difficult to see how the truth of this contention can be denied. Dr. Haldane urges physiologists to revise their notions about matter, but physiologists in general never trouble themselves about such things because they suppose themselves to be above 'metaphysics' when in fact they are only a very little above it—being up to the neck in it. They have been taking matter as ultimate but without being very clear about what they meant by it, and they have been trying to make the facts about organisms fit this notion. And many of them do fit tolerably well to encourage them in the hope that ultimately they will all fit. Meanwhile physics, upon which biology is to be based, is rapidly freeing itself from this notion aided by epistemological criticism. If this criticism is just—and it seems to be—then it should have important consequences for biology, but there does not seem very much hope of those consequences being developed or worked out if the majority of physiologists persist in their present complacent adherence to tradition.

I do not wish to pursue Dr. Haldane's view much further in detail in this place, but a few more of his sayings may be introduced before we pass to other mechanistic methodologists. Dr. Haldane wants to take the concept of the organism as a unit of realised activity[1] as ultimate instead of 'inert matter'.

[1] I do not profess to understand exactly what this means, but compare the same expression in the passage from Whitehead quoted below on p. 421.

The aim of biology then becomes he says :

> ' to trace in detail, and with increasing clearness, the organic determination which the ground conception postulates.'

What then, does he mean by organic determination? On p.86 of the book we have been discussing, he says, that it means :

> ' determined as in definite relation to the whole functional and structural activities of the organism, and not merely dependent on specific and one-sided conditions. . . .''

That is to say that the whole enters always into the determination of the activities of the parts. This is just the denial of the proposition of Claude Bernard which we have already referred to. And if this contention is true we can see that what is left out by orthodox physiology according to Dr. Haldane is not just something which is unimportant or beyond the scope of scientific method but something of fundamental significance which, as already suggested, may actually be operative in all biological thought even although it is not consciously recognized. Professor D'Arcy Thompson, in a review of Dr. Haldane's book, *The New Physiology* in Mind N.S. Vol. XXVII, p. 359, seems to have missed the point just as Professor Marshall does but he comes within an ace of realizing it when he says : ' it is not biology which he (Dr. Haldane) is trying to reform, but the current thought of the world '. And of course Dr. Haldane (in company with a number of other people) is trying to reform the current thought of the world. But why not? Is that thought infallible? If so what are its claims to infallibility, and are they any better founded than other claims of that sort? If it is not so immodest as to claim infallibility then it can hardly object to reform. And if what the reformers have to offer is ' difficult ' or ' by no means clear ' that is hardly to be wondered at when we recall all the factors mentioned in the last chapter which play into the hands of conservatism, and when we remember how difficult it is to overcome a well-embedded system of thought, however well one may appreciate its deficiencies. The present system was not built up in a day, nor was it constructed to deal with present day problems. Accordingly it need not surprise us if knowledge has outgrown its earlier metaphysical clothing. But if we cling exclusively to the old because it is still useful or because any other alterna-

tive is difficult we shall continue to stagnate. The history of the first Renaissance illuminates the birth struggles of the one now in progress. The opponents of reform at the present day are repeating the same kind of obstructionism as was practiced by the adherents of the Aristotelian view of the world. Not with any deliberate evil intent but simply from a failure to appreciate either the need for reform or the tentative efforts of the reformers.

From what has been said it is clear I hope that, whatever may be the truth about Dr. Haldane's contentions, Professor Marshall has totally misunderstood him, and is quite wrong in his confusion of biological concepts with teleological ones in the sense in which he appears to understand them. That he does make this confusion is evident from the fact that he challenges Dr. Haldane to ' put forward any purely biological or teleological theory of heredity which will conform to the conditions of a true scientific hypothesis '. He has himself given an instance of a purely biological theory of heredity when he mentions Mendelism, and this is generally regarded as a true scientific hypothesis, and also as being free from ' teleology '.

There is only one more point in Professor Marshall's paper to which attention need be called. He says (p. 70) quite rightly that the answer to the original question with which he began depends upon what we mean by biology.

> ' Do we mean the biology which is known to the worker in the laboratory and the observer in the field, or do we mean a biology which abstracts less and includes more, which embraces all the categories of psychology as well as those of teleology and the more concrete forms of knowledge, which inquires into the purposes of things, and their relation to the ultimate reality ? If that is what is meant by biology, it is no part of natural science, and its relation to the limited field of inquiry which the scientific worker knows under the same name is in its essence no different from its relation to chemistry and physics. . . . All of these sciences deal with abstractions and teleology has no place in any of them . . . "

I imagine that no one would wish to deny this for a moment but what has it to do with the question ? The original question was ' whether the categories of biology are ultimately reducible to those of chemistry and physics ' and most people on reading this would take the word biology to mean that which is known to the worker in the laboratory and the observer in the field. What Professor Marshall seems to suggest now is

that there is no stopping between the mere collection of data in accordance with a 'policy' phenomenalistically interpreted on the one hand, and metaphysics *überhaupt* on the other. But this is the standpoint I have all along been contending against. Surely it is legitimate to speak of theoretical biology or theoretical physics, etc., indicating thereby that each science not only seeks to discover facts with a view to utility, but also to attain to a general comprehension of the whole of its sphere, without prejudice, however, to still wider questions. And if metaphysics is not to make use of these general interpretations of the sciences regarding nature I fail to see where it is to get its information about nature from. But if the sciences content themselves with a purely utilitarian phenomenalistic scheme I also fail to see how metaphysics can put any reliance upon them or make any use of them at all.

It does not seem then that there is anything at all conclusive for our purpose to be obtained from Professor Marshall's paper, and for the following reasons: (1) he approaches the question solely from the standpoint of the physiological investigator, and we are concerned with the standpoint of the theoretical biologist who recognizes that there are other branches of the science, e.g. genetics and embryology, etc. (2) he takes a purely phenomenalistic view of science which is here rejected; (3) he interprets 'mechanistic' to mean 'using the concepts of physics and chemistry' and contrasts this with 'biological or teleological', but without justifying the identification of the latter two terms in this way.

An example of ontological dogmatic mechanism is afforded by Professor Sir E. Schafer's presidential address to the British Association in 1912. By a mechanistic explanation he simply means one in terms of the notions current in chemistry and physics, especially the former. Professor Schafer does not discuss the question from the methodological standpoint at all and therefore his view falls outside the scope of this discussion. But it is worthy of mention here as an illustration of the extraordinary simplicity with which biological problems present themselves to the physiologist. He simply picks out all those characteristics of organisms which can be imitated in the inorganic world, and omits all the

difficulties which arise when we try to push the analogies a little further, until we wonder how anyone could have come to suppose that organisms presented any problems at all. Moreover the concepts of physics are taken on their face value as quite concrete and exhaustive in their own sphere. Such a point of view can either be accepted or rejected as a whole: there is no scope for discussion. Ontological mechanism of such a simple rock-ware kind really belongs to the realm of religious enthusiasms like some ontological vitalisms and is therefore beyond the reach of criticism. It is difficult to avoid the conclusion that such extremes are reached more by way of a 'will to believe' than by the simple desire to find out what is the case.

We will leave the physiologists for the present and turn to the views of the late Dr. Jenkinson which are expressed in the last chapter of his *Experimental Embryology*.[1] On p. 297 he writes:

> 'By exact observation and crucial experiment, utilizing every canon of induction, the facts of development are to be brought under wide general laws of causation, which will be in the first instance physiological laws—of response to stimuli, of metabolism and growth: by means of these laws we can predict and our predictions can be verified.'

Thus Jenkinson believes that biology should proceed by induction to reach causal laws expressed in biological terms. This seems a much more reasonable methodological procedure than the extremes advocated by the previous writers. He then proceeds as follows:

> 'The thought process cannot, however, end here. Ultimately—as we believe—it may be possible to state the widest generalizations of biology in chemical and physical, and these again in purely mechanical, terms. Thus evolution of form in the individual as well as the larger evolution of form in the race become but the final terms in a far vaster cosmic process, from "homogeneity to heterogeneity."'

It is plain that the differences between this author and the others are correlated with the fact that he is an embryologist. This makes him more sympathetic to physiological or biological laws as a first objective, but it also leads him to introduce the notion of development. He appears to be an ontological mechanist. There is no taint of phenomenalism in his views. He interprets science quite realistically. He

[1] Oxford, 1909.

speaks as though mechanics (in the strict sense) were the last word of physical science, and it must be remembered that, when he wrote, the revolution in physics had not progressed so far as it has at present. Being an evolutionist he thinks of the hierarchy—mechanical, chemical, and biological as though it were a kind of genetic series. This of course is an extension of the notion of evolution beyond its original biological meaning. He thus introduces the notion of origin alongside that of causation. He says:

> 'Phenomena are thought out in terms not of origins merely but of one origin, and that one origin is the only mystery that remains. This unification of the sciences always has been, and must still remain, the dream and the faith and the inspiration of the scientific man, and could such an edifice of the intellect ever be realised the task of science would have been completed.'

There is a curious blending of methodology and metaphysics here with the emphasis on the metaphysics. Evolution, interpreted metaphysically not as a biological process but as one involving the whole universe, is made the basis of a methodology whose business it will be to proceed in the opposite direction—from 'heterogeneity to homogeneity'. Thus there is no 'mystery' in the process, but only in the beginning. Jenkinson puts forward this scheme as a dream and a faith, but dreams do not always come true and faith is sometimes misplaced. Moreover how can we tell how many mysteries will remain when the goal is reached? Clearly we shall never find an answer to our question about alternative methods of interpretation in biology if we proceed in this way. The question is settled in advance by speculative methods, but science is not concerned with dreams.

It is curious to note the way in which Jenkinson assigns quite a different place to philosophy from that given to it by Professor Marshall. He says:

> 'But where science leaves off, there philosophy begins, and it is for philosophy to attempt the solution of this last mystery of all.'

But if this is the business of philosophy it will clearly have to remain idle for a long time—until the dreams of the man of science come true. Because even if we do know that only one mystery will remain there does not seem to be much hope of ever saying anything about it, certainly not until the intervening stages have been safely passed over. Moreover if the man of science is to throw up the sponge when he gets to

mechanics there does not seem to be any reason why a philosopher should fare any better in face of the awful mystery that remains.

Further cogitation leads Jenkinson into a difficulty which always arises when evolution is blended with mechanics:

> 'Philosophy cannot rest content in an endless regress of cause and effect, and a first supreme cause, first in time that is, is metaphysically out of the question. An original homogeneity is equally unthinkable, for out of a system all whose parts are absolutely alike, by no imaginable process could any heterogeneity ever be evolved.'
>
> 'That first simplicity must have contained *potentia* all that has since developed out of it, it must have possessed a structure, an arrangement of parts such that the end which it has realized, or is to realize, would be what it is or will be . . .'

It looks therefore as though should the dreams of science come true philosophy will have to begin all over again. For what seemed to be evolution was not evolution, and what was taken to be homogeneous turns out to be anything but it, indeed more complicated than anything that has since appeared. It seems clear that Jenkinson has taken the simple working notions of science, such as causation and evolution, and incautiously extended them for metaphysical purposes into imaginary regions about which we really know nothing, 'the universe' being conceived as a fertilized ovum. He *seems* to contradict what has gone before when he says on p. 299 that knowledge through 'material and efficient causes' has to be 'rounded into a whole' through a knowledge of the 'final cause', which he says is just as much a *vera causa* as the former on account of our ignorance about the causal nexus. Jenkinson says that it is only because of our desire to predict that we have come to restrict the use of the term cause to the antecedent. He also says that 'a purposiveness' is an 'unmistakable characteristic of the functions of living things' but a little further on he says that this purposiveness is 'only the expression of our inability to comprehend their beginnings except in terms of their ends'. It is 'relative to us' but not therefore any 'less real', but nevertheless it is a characteristic which 'still remains when those functions have been expressed in terms of the chemistry of the proteids'. What is one to understand by all this mystification when what is conceded in one proposition is denied in the next? When a thing has been 'expressed in terms of the chemistry of the proteids'

it is not altered thereby in any way. What then is meant by saying that it 'remains'? Next there follow a series of assertions which seem to contradict either one another or those made previously.

> 'Biology, then, although built upon the ultimate conceptions of chemistry and physics, has yet peculiar features of its own. . . . A survey of the whole hierarchy displays to our view a series in ascending order of complexity; each member of this series has its own ultimate conceptions . . . each endeavours to translate its own ultimates into those of the science below: a translation however, which, be it never forgotten, leaves the reality of the original undestroyed.'

What precisely are we to understand by all this? A conception is said to be ultimate when it cannot be reduced to any other one. If biological concepts can be reduced in this way then they are not ultimate, but if they cannot be so reduced then they *are* ultimate. But whatever happens to the *concepts* has nothing to do with that *of which* they are concepts, this seems plain enough not to be in danger of being forgotten. Each stage in the 'cosmic process' says Jenkinson, possesses peculiar features which are 'simply the outcome of an increase in complexity'. If this is the case then the biological conceptions of those features were in no sense ultimate, and, after the 'translation', which is simply a process of increasing knowledge, the only reality that remains is the reality which was there before and has been since the 'first simplicity' namely, according to Jenkinson, 'intra-molecular atoms'. But if this is the case we have given no account of the 'peculiar features', which seem to have dropped out of the scheme altogether. This is an instance of the old difficulty exemplified by the 'secondary qualities' all over again. It seems perfectly clear that we have simply and quite legitimately ignored the 'peculiar features' in order to arrive at those that are not peculiar but are pervasive over all the 'stages', but what is *not* legitimate is to forget this and suppose that the peculiar features have been embraced in the more abstract scheme. The reason why they 'remain' is because they have not entered into the scheme at all, but have only been forgotten. Clearly then we require *other* schemes in order to take them in. This, I understand, is recognized in the more recent schemes of emergent evolution which simply accept the peculiar features and do not attempt to 'explain' or 'reduce' them. Thus although Jenkinson does not discuss the methodological

point of view his views have offered a good example of the above fallacy resulting from not understanding the nature of abstraction. Jenkinson, in fact, admits that what has been accomplished is not what the original scheme supposed it to be when he says :

> ' Nevertheless, the facts with which each science starts, the facts which come first in the order in which knowledge is acquired, do not become wholly merged in those simpler facts into which they are at each stage translated ; when the translation has been accomplished the original still remains.'

But if this is admitted then it cannot be said that the difference between one level and the next is simply one of difference in complexity, nor that only one ' mystery ' remains. The differences between the various levels are genuine ultimates and there is a ' mystery ' at *each* stage, if by a mystery you mean something which is not embraced by the more pervasive and abstract explanatory scheme. Jenkinson seems to have got into a muddle by trying to have it both ways. He wants to have evolution with a genuine series of successive phases with ' peculiar features ', but at the same time he wants a beginning which was at once ' homogeneous ' and yet containing potentia ' all that has since developed out of it '. He wants the concepts of one science to be both adequate to that science and yet to be reducible to those of another more abstract one.

Finally, it seems impossible to reconcile what Jenkinson says about evolution with his last paragraph where, after saying that the mind has been ' developed out of and conditioned by matter ' and is the ' last term and final cause of the whole process ' he says : (p. 301)

> ' And in this mind, last in time but first in thought, a larger philosophy will perceive not only the end towards which, in time and space, matter strives, but the Understanding which, itself eternal, imposes the forms of space and time upon that Nature which it makes.'

Now, if we are to perceive in mind the Understanding which (1) is eternal, and (2) *imposes* the *forms* of space and time upon nature and (3) *makes* nature, then how can that mind *possibly* have evolved ' in time and space ' from ' matter ' ? What can it mean to say that the mind is ' last in time ', and how in the name of fortune are we to make sense of the statement that it is the end ' towards which, *in time and space*, matter *strives* '?

Let us leave these turbid Oxford metaphysics and turn to something less ambitious. We have not gleaned anything much about mechanical explanation which is very clear, although we have noted some of its implications, when coupled with evolutionary notions, which may prove illuminating. The upshot of it all seems to be that Jenkinson means by a mechanical explanation one in terms of the classical mechanics, as was the case with Verworn, but it is to be a guiding inspiration, and does not claim to be 'self-evident'. It seems clear that if we adhere to known facts the 'dream and faith' as depicted by Jenkinson has very little to do with real biology. These aspirations are the result of the irresistible tendency of thought to run on in its conceptual world to propositions which carry us into a transcendent world beyond the boundary of a 'possible experience'. Such speculations, while they may have some positive value as furnishing a motive for research, also have their negative side, because they rest on the assumption that nature has no surprises in store for us, as physics seems to have believed during the last century. They suggest that we know all that there is to know *in principle*, and that the future history of science will only tell the story of the working out of details. This is a dangerous doctrine especially in a young science such as biology. Much the same attitude prevailed after the Darwinian publications, and everyone knows how detrimental it was even to research except along certain restricted lines. Thus Bateson wrote: 'Darwin's achievement so far exceeded anything that was thought possible before, that what should have been hailed as a long-expected beginning was taken for the complete work.' Every successful scientific doctrine seems to have this double aspect. On the one hand it opens up new avenues of thought and on the other it seems to have a conservative and blinding influence tending to restrict further advances not in conformity with it. And it is just *possible* that the mechanistic ideal as commonly interpreted, if it is to be adhered to as the *exclusive* mode of interpretation in biology as some people wish it to be, may have a similar blinding influence. It is for this reason that it is worth while to devote a little care to trying to understand it. And in any case to found biological thinking on the metaphysical assumptions of mid-Victorian physics and the philosophy of Mr. Herbert Spencer is to found it upon a sand castle of which little has been left by the rising tide of twentieth

century criticism. But when we talk too naïvely of forces and matter and causes that is precisely what we are doing.

Professor E. B. Wilson makes occasional references to mechanical explanation in various places in his great book on the cell. But it is not easy to discover his views. He approaches the question from the standpoint of the investigator rather than from the point of view of one who wishes to understand the theoretical value of the mechanical explanation. Sometimes he insists on its inadequacy, and at others he insists equally that we have no other alternative. He uses the expression in more than one sense. On p. 635, in discussing the expression 'chemical machine' used by Loeb, he says:

> 'Even the most superficial acquaintance with the cell-activities shows us that this conclusion cannot be taken in any crude mechanical sense—the difference between the cell and even the most intricate artificial machine still remains too vast by far to be bridged by present knowledge. Nevertheless, we accept the hypothesis that the difference is one of degree rather than of kind because it has proved itself fruitful in discovery and has kept us moving in the right direction.'

Now here is a new sense of 'mechanical explanation' in which it seems to mean interpreting the organism on the analogy of a humanly constructed machine. Professor Wilson draws attention to its crudity but defends it on heuristic grounds. We shall have occasion to examine the consequences of this meaning of 'mechanical' later, meanwhile we can consider some other expressions of opinion by Professor Wilson. On p. 670 he refers to the shortcomings of purely physical and chemical explanations as understood by Professor Marshall. This is interesting in view of the fact that Professor Wilson is a cytologist. He says:

> 'We cannot hope to comprehend the activities of the living cell by analysis merely of its chemical composition, or even of its molecular structure alone. Many investigators . . . have tried to formulate vital activities in terms of the properties of large molecules . . . and this conception has rendered important service in physiological analysis. Modern investigation has, however, brought ever-increasing recognition of the fact that the cell is an organic system, and one in which we must recognize the existence of some kind of ordered structure or organization.'

Professor Wilson also points out that this is implied in the view of Loeb. It seems then that in addition to the chemical and physical aspects emphasized by Professor Marshall and the evolutionary standpoint of Jenkinson there is something else about organisms brought forward by the cytologist, namely organization. Professor Wilson seems to suggest that this cannot be adequately dealt with from the chemical analytical point of view but much later in the book he seems to contradict this. On p. 1037 he says, again speaking of organization:

> 'As yet we have no adequate conception of this organization, though we know that a very important part of it is represented by the nucleus. . . . Nevertheless, the only available path towards its exploration lies in the mechanistic assumption that somehow the organization of the germ-cell must be traceable to the physico-chemical properties of its component substances.'

Evidently organization is important and if we have no adequate conception of it we ought to set about getting one. In some parts of his book Professor Wilson seems to take a more phenomenalistic attitude towards explanations. Speaking of certain 'corpuscular theories of heredity' he says (p. 1113):

> 'It is, I suppose, theoretically possible to consider such hypotheses as nothing more than a convenient fiction or algebraic symbolism, a kind of ideal mental model by means of which the genetic facts may conveniently be grouped.'

And on p. 1117:

> 'Existing mechanistic interpretations of vital phenomena evidently are inadequate: but it is equally clear, as someone has said, that they are a "necessary fiction." Knowledge will be advanced most surely by assuming that the problems of the cell can be solved by converging upon them all our forces of observation and experiment.'

Thus Professor Wilson also appears to write from the standpoint of the investigator to whom 'knowledge' means facts and to whom therefore any device, even if it is only a fiction, is acceptable so long as it justifies itself heuristically. The last sentence is characteristic in omitting *thought* from among the requisites for the advance of knowledge. But if biology is ever to be more than a 'medley of *ad hoc* hypotheses' thought will be just as necessary as observation and experiment. It seems clear that Professor Wilson uses the expression 'mechanistic explanation' in two principal senses—sometimes to mean an explanation in terms of the concepts of physics and chemistry, and sometimes to mean an explanation

on the analogy of a machine. But there is no talk of 'reducing' everything to mechanics as there was in the case of Verworn and Jenkinson. Professor Wilson's opinions are characterized by restraint and lack of dogmatism, and he is evidently in doubt about how exactly we are to regard such interpretations. He nevertheless believes that we have no other alternative. And his remarks, being guided only by the heuristic point of view, have not been of much help towards the solution of the theoretical question regarding the sufficiency and inevitableness of methodological mechanism.

There remains one more sense in which the term 'mechanistic' has been used in current biological literature. This is illustrated by the following passage from a book by Mr. de Beer:

> 'This in no way commits one to the "materialistic" idea of life. Neither does it mean that life is nothing but physics and chemistry. What this point of view does stand for is that whatever the processes of life may be, they work in an orderly way, producing similar effects under similar conditions. Steering between "materialism" and "vitalism", this conception has become known as mechanistic.'[1]

It seems clear from this that all that is here meant by mechanism is the belief in the 'law of causation' or the 'uniformity of nature'. This is commonly regarded as a necessary methodological postulate of natural science. And being a methodological postulate it makes no assertions about the nature of the processes studied, but merely asserts that they take place according to law, or 'work in an orderly way.' But it is not quite clear how this can be regarded as offering an alternative to materialism or vitalism. One might as well speak of socialism steering between quakerism and bimetallism. Mechanism in this sense is not a biological doctrine but a methodological postulate. Materialism is the name either for a metaphysical theory about the universe, or for an explanation employing the concept of matter. Vitalism again professes to be a doctrine about organisms—a metaphysical theory in the sense of ontological, not about things in general but only about organisms. If we are to find a third alternative to vitalism on the one hand and a materialistic theory on the other it cannot be a mere methodological postulate which tells us nothing about organisms, but

[1] *Growth*, London, 1914, p. 113.

merely asserts what it is believed has to be assumed if we are to study them scientifically. I doubt whether in the case of biology (in some departments at least) it need even be called an assumption. It is surely a matter of observation that the developmental processes, for example, 'work in an orderly way'. It is not a question of finding regularity but of analysing, and finding an interpretation of, it which can be generalized. Nevertheless the above meaning of 'mechanism' is one which is commonly used. Psychologists for example, speak of the 'mechanism of a neurosis' referring thereby I suppose to the ordered 'structure' of psychical processes. They assume that there is some such orderly structure and call it a mechanism without implying anything further about the ontological nature of those processes. Thus the term may be used for the methodological postulate that there is some sort of order, and then it may be applied to that order itself.

I believe I have now noticed the principal senses in which the expression 'mechanical explanation' is actually used by biologists, and the grounds upon which adherence to them is defended. These principal senses are as follows: (1) as meaning an interpretation in terms of the classical mechanics. With Verworn we saw that the necessity of this explanation in biology was held to be 'self-evident'; but with Jenkinson it was a 'dream and faith'. (2) The second meaning was that of Professor Marshall which seemed to mean simply any explanation which uses the concepts of physics and chemistry. This was held to be sufficient because it had been successful heuristically in the past, because it was assumed that the only alternative was 'teleology' (undefined), and because what went on in the parts of organisms when isolated was assumed to be what went on in them when in the whole. Professor Wilson also employed the term in this sense, i.e. as a name for any interpretation in physical or chemical terms. (3) The third meaning seems to signify any interpretation on the analogy of a machine in the common sense. Professor Wilson also used the term in this sense. Loeb speaks of the organism as a 'chemical machine'. (4) In the fourth meaning, that of Mr. de Beer, the term was used for a methodological postulate which asserts that the object of study is in some way

orderly, and we saw how this term may then be extended to that which is thus assumed to be orderly but without any further metaphysical assumptions about its 'nature.'

I now propose to examine some of these meanings in more detail, beginning with the first, and it is necessary to inquire what is understood in physics by a mechanical explanation in this strict and proper sense, in order to see how biology stands in regard to it. Fortunately Dr. Broad has contributed a very valuable and thorough analysis of the various kinds of mechanical explanation for which biologists should be thankful, and the reader may be strongly recommended to consult the original.[1] I need only draw attention to the chief points. It should be said at once that Dr. Broad does not profess to discuss the question from the standpoint of the biologist but simply to explain what the mechanical explanation *sensu stricto* involves. Early in the paper Dr. Broad says that:

> "An explanation of any phenomenon always involves two factors: general laws and a specified set of entities subject to these laws. For what is to be explained is a definite particular state of affairs, and you cannot explain this merely by a set of general laws."
>
> ". . . laws assert correlations between attributes. What has to be explained is some more or less specialized instances of correlated attributes. Laws alone will not explain this: one specialized instance can only be explained in accordance with a general law by another specialized instance."

There will, therefore, be two questions requiring an answer in reference to mechanical explanations: (1) What is the particular nature of the entities employed in them? and (2) What is the peculiar nature of the laws? Dr. Broad then goes on to explain that all mechanical explanations imply that 'the phenomena under discussion obey either Lagrange's equations or some substitute for them which approximates indefinitely to them for ordinary velocities'. Lagrange's equations constitute a statement of the laws of motion in a concentrated form and in terms of quantities which can be actually observed and measured. Dr. Broad distinguishes five different kinds of mechanical explanation involving Lagrange's equations, of which two are 'macroscopic' and the rest involve an appeal to microscopic entities. Now I do not suppose that any biologist regards the mechanical explanation

[1] *The Mechanical Explanation and its Alternatives*: Proc. Arist. Soc., Vol. XIX., 1919, pp. 86-124.

in the *macroscopic* sense as at all applicable to biological phenomena as a whole, and we need not therefore consider it. Dr. Broad himself says:

> 'In practice all the more rigid forms of mechanism require a microscopic analysis of phenomena, but this is simply because they are palpably false if asserted to apply directly to all macroscopic phenomena. . . . Again, macroscopically, there are laws of nature, which are not capable of a mechanical analysis, e.g. the laws of electro-magnetics. Hence pure mechanism is certainly false if it be asserted to hold macroscopically of everything in the world. Thus the connexion between homogeneous or pure mechanism, and microscopic explanation is that, if these forms of mechanism be true at all, they must be true microscopically, since they are certainly false macroscopically.'

There is no doubt that what physiologists mean when they depict their ultimate goal to be mechanical explanation in a strict physical sense is some form of microscopic mechanism. Now what Dr. Broad calls 'heterogeneous microscopic mechanism' requires that:

> 'the obedience of the non-geometrical generalised co-ordinates of a system to Lagrange's equations is due to the correlation of the macroscopic phenomena with microscopic transactions, which obey these equations in a form in which all the generalised co-ordinates are geometrical magnitudes or times.'

As far as I can discover no physiologist has made use of the mechanical explanation in *any* of Dr. Broad's senses,[1] and if any one wishes to set about improving on this state of affairs he should consult Dr. Broad's paper at once. It is particularly instructive to note what Dr. Broad says about chemistry, for as he says, this question of the sufficiency and necessity of mechanical explanation arises also in regard to chemistry.

> 'I do not think that pure mechanism deserves to shine in the light reflected from the successes of the atomic theory in chemistry or of the electron theory. The atomic theory contradicts homogeneous mechanism and makes no assumption in favour of pure mechanism.[2] It is useless to say that perhaps the differences

[1] I took the precaution of consulting Prof. A. V. Hill on this point and he kindly gave me the following reply: 'I have never heard of any application to physiology of Lagrange's equations, but they always seemed to me, when I studied such things, very pleasing and beautiful. Some day perhaps when physiology is more of an exact science than it is at present, we may hope to find some use for them. Within my limited knowledge, however, at present, I have never heard of their application.'

[2] By pure mechanism Dr. Broad means a form in which 'all variables would be geometrical magnitudes or their first derivatives with respect

between an atom of oxygen and one of hydrogen are merely differences between the number and configuration of two different groups of precisely similar particles, whose laws are mechanically analysable. Perhaps they are. But since chemistry has no need to make any assumption on the question one way or the other, the success of the atomic theory up to the present can have no tendency to support this view, and therefore can reflect no credit on homogeneous or pure mechanism. . . . I think we are justified in saying that the possibility of dealing scientifically with a given region of phenomena does not imply that it must be known to obey even microscopically the more rigid forms of mechanism. And if anyone says that its explicability must depend on its actually doing this, whether the fact be known or not, he is asserting a pure dogma, for which, from the nature of the case, there can be no evidence.'

I think it fairly safe to say that so far as present day biology goes mechanism in any of these strict senses is of very little significance, and when biologists talk about mechanical explanation as an actual working instrument they do not have anything like this in mind. Such strict mechanical explanation may be a 'divine far off event' to which the whole (biological) creation moves, and it is very desirable that some biologists should work with the aim of making use of such devices. But it does seem to me to be the greatest folly either to discredit the attempt to explore other ways of thinking, or to allow such assumptions to be treated metaphysically and so to influence biological theory until that event is realized. If the latter course is taken we shall simply be repeating the mistakes of previous generations.

We can turn now to our second meaning of mechanical explanation, in which it simply meant using the concepts of chemistry and physics. It is probably in this sense that the term is most commonly used by physiologists when they are talking about the present state of biology and not some remote hypothetical future state. There are three comments to be made about the view represented by Professor Marshall. First that controversies relating to this topic are apt to be rendered futile by an equivocal use of the terms 'physical' and 'chemical'; secondly that some important branches of

to time, and all the macroscopic differences between one kind of matter, or one state of matter, and another, would be those based on differences in respect of these variables among qualitatively homogeneous electrons," etc. See *op. cit.*, p. 101.

physiology, e.g. neurology, still use biological concepts exclusively or almost so. And thirdly that difficulties arise for this view when those branches of biology are taken into consideration where organization is of importance, as we saw from the comments of Professor E. B. Wilson. These three difficulties are related. I will first explain what I mean by the equivocal use of the terms 'chemical' and 'physical'. Confusions arise I think from a failure to distinguish chemical entities from chemical concepts. A lump of iron, for example, is a chemical entity, and the word 'iron' stands for a chemical concept. But suppose the iron has the form of a poker or a padlock, then although the iron is still chemically analysable in the same way as before it cannot still be fully described in terms of chemical concepts. It now has an organization above the chemical level. In the same way an organism is a physical entity in the sense that it is one of the things we become aware of by means of the senses, and is a chemical entity in the sense that it is capable of chemical analysis just as is the case with any other physical entity, but it does not at all follow from this that it can be fully and satisfactorily described in chemical terms. It is not at all difficult to see why so many physiologists take this 'physico-chemical' view of the organism when so much of physiology is capable and only capable of chemical study. The physiology of nutrition, for example, is largely occupied with purely chemical changes requiring only chemical concepts. But when we extend our view to further implications of the question of nutrition we soon get into regions where this is no longer the case. There is the question, for example, of how the food gets into the stomach in the first instance, and how it comes about that meat gets into some stomachs and only grass and never meat into others. Abstraction is made from all this when the chemistry of digestion is under consideration and quite rightly, but all these further questions are clearly involved and are legitimate biological questions, but it is not at all easy to see how far we are justified in saying that only chemical and physical concepts should be employed in their interpretation. These then are some of the facts that lead such writers as Professor Wilson and Dr. Haldane to insist on the importance of organization, and we shall try to inquire more closely into their implications when we come to discuss that notion.

When we come to the third meaning of 'mechanical explanation' according to which the organism is to be interpreted according to the analogy of a humanly constructed machine we reach a meaning which is frequently confused with the foregoing and which is important because it does attempt to overcome the difficulty mentioned in the last section. This view underlies all serious attempts to arrive at a general interpretation of the organism which are at all adequate, and it underlies also both the vitalistic and the materialistic views. Any such theory has to give some account of the fact that the organism is in some sense a single individual thing possessing a greater or less degree of independence according to its evolutionary status. This the 'physico-chemical view' as above described does not attempt to do, and this deficiency is what the machine theory tries to remedy. Those biologists who are not interested in such aspects of the subject naturally never feel the need for taking any account of it in their theories. For them a purely analytical procedure, using only physical and chemical concepts, naturally seems perfectly adequate. We noted how Professor Marshall failed to understand why Dr. Haldane insists that we must 'keep the whole organism in view'. Now the merit of the machine theory lies in the fact that it does attempt to deal with the whole organism because a humanly constructed machine is a whole—a single individual thing. It thus has the necessary unitariness which enables it to provide an analogy to the living organism. If we examine the vitalistic theories we find that they also usually employ the analogy with a machine, but, in a sense, they carry out the analogy more thoroughly, because their machine has a mechanic. The materialistic machinists want to have a machine without a mechanic. Their problem is to show how it is possible to have a machine which comes into existence, runs, repairs and regulates itself and finally divides into two machines, without a mechanic. This is the problem which confronts the ontological mechanist who says that the organism is in some sense a machine, and the real point at issue between him and his vitalistic rival is just this one regarding the question of the mechanic. Even the methodological mechanist who honestly tries to grapple with the general problems of biology, and is not content to ignore them because his own

particular province does not involve them, is driven to seek aid from the analogy with the machine. And when the analogy begins to present difficulties it then becomes time for the methodological machinist to consider, if he does not like to postulate a mechanic, whether it is not desirable to examine the analogy itself more closely and to look about for a better way out. It is well to remember that Descartes, who was the founder of the machine theory, had no difficulty over this question of the mechanic because he regarded God as the mechanic who had created all the machines once and for all. But in modern times the situation is not so easy because our machines are required to have evolved, and so we get into the difficulties which we saw presented themselves and were not clearly resolved in the discussion in Jenkinson's book on embryology. It is the difficulties introduced by developmental notions which lead to the antithesis between preformation and epigenesis. Thus the machine theory, which attempts to take account of the facts of organization, gets into further difficulties when division and development have also to be dealt with, and these difficulties may simply be a consequence of the fundamental inadequacy of the original analogy. Moreover this analogy has always been carried out on the basis of the traditional materialistic metaphysic of natural science, and if that metaphysic is given up it may be easier both to understand the defects of the analogy, and also to find some better alternative.

Another question which has already been referred to is the question of the limits of the mechanical explanation even in the physical sciences. The fact that they are not exhaustive even in the physical sphere is often forgotten too when they are carried over into biology. But the old difficulty of the secondary qualities always crops up on the materialistic view because this view never made any attempt to deal with it. If we adhere to this view we have to admit other types of law over and above the mechanical in order to connect the mechanism with the secondary qualities and with the minds that observe them. Dr. Broad, who discusses this difficulty in the paper already referred to, says:

> 'any theory which counts the microscopical mechanism as real and not as a mere mathematical construction must recognize three different kinds of laws: (i) those obeyed by matter in its mutual action; (ii) those according to which matter affects mind; and (iii) those of minds and their states.'

There is little doubt, I think, that this further complication is usually not understood and the mechanical view is thought to be more simple and exhaustive of what there is to know than is actually the case.

It will not be necessary to devote much attention to the vitalistic theories for two reasons: first, because they do not lack critics, and secondly because there does not seem to be much to be said in their defence. At the same time it may be proper to point out that a good deal of the criticism is rather beside the point. It is not possible to refute ontological vitalism. The fact that it offends our monistic predilections is not an argument against its truth. It is quite impossible to say that there are not such entities as entelechies in the world. The chief objection from our point of view is that even their most staunch defenders are able to say so little about them. They can tell us what they are *not*, but from the nature of the case it is not possible to say anything positive. Entelechies are thus in the unfortunate position of the 'external world' of some of the phenomenalistic writers. We are told that they exist and that is about the limit of our possible knowledge. They are thus extremely uninteresting entities especially from the standpoint of natural science. To invoke entelechies as *agents* at the point where the materialistic machinist gets into difficulties has not proved to be of any value from the methodological standpoint because it is too successful, too general and gives us no light upon the particular case. Entelechies are in just the same position as energy when the latter is regarded as 'the cause of changes in matter',[1] and such an appeal to imperceptibles is contrary to the present tendencies of scientific thought. We want to see how far we can get without making such appeals. It is easy to understand why vitalistic theories have been unpopular when we run over the list of traditional demands and postulates of natural science but I do not wish to suggest that failure to satisfy such demands is a sufficient ground for rejecting them. And it must not be forgotten that the vitalistic writings have their valuable side. They represent an adventure in thought —the exploration of a possibility—and biologists should be

[1] See above, p. 173.

grateful to Driesch for the patience and thoroughness with which he has carried out this explanation. If we find that this path leads us to an *impasse* beyond which it is impossible to go it need not be concluded that nothing has thereby been accomplished. There is a great deal to be learnt from the writings of Driesch because they deal with those aspects of the organism which are omitted by the other writers.

When we turn to the psycho-biologists we do not appear to fare any better. They appear to want to extend psychological notions reached by introspection to the interpretation not only of the behaviour of lower organisms but also of the morphogenetic processes, and in so doing they are confronted with difficulties of the same type as are met by vitalists of the Drieschian kind. But they also have the merit of calling attention to aspects of the organism which any adequate theoretical biology will have to take into account.

From the point of view of the logic of biology the anti-mechanistic criticisms amount at bottom to this: In your treatment of the organism, they say, you have omitted something. And to this there appear to be three principal types of answer: (1) The something omitted is beyond the scope of the scientific method. This answer leads to a narrow interpretation of the scope of science which wishes to identify it with mathematical physics, and calls every other kind of knowledge 'philosophy'. (2) The something omitted is illusory or 'unreal'. This is the method which was followed by physics in dealing with the secondary qualities. From the purely methodological standpoint it is unimpeachable, but from the theoretical point of view it has the disadvantage that the 'unreal' ultimately returns upon you like a boomerang, because nothing is banished from the universe by being called unreal, any more than it would be by the utterance of a magical incantation. (3) The something omitted will be embraced by our scheme when knowledge is sufficiently advanced. This type of answer in so far as it is an expression of an unbounded optimism and faith in the possibilities of one exclusive point of view in the study of nature also has heuristic merits in providing an urge to investigation. But both from the logical and theoretical standpoints it has the disadvantages of dogmatism since it closes the door to the exploration of other possibilities.

We now have to try and summarise the progress so far made towards an understanding of the antithesis between vitalism and mechanism. The situation is complicated as we have seen by the existence of much diversity of opinion and by the use of the term 'mechanical' in diverse senses, so that the issues are by no means clear cut. And the diversity of opinion itself depends on a diversity of factors. Consider first the historical aspect. Mechanistic theories are the outcome of a long and successful line of thought developed, in the first place, in relation to the problems of the physical sciences. This line of thought has two great advantages as compared with any possible rivals. In the first place it is heuristically successful, and secondly it arose out of common-sense thought, and, superficially at least, does not clash violently with common sense and the 'familiar', nor does it, for that reason, make any great demands on our intellectual powers. We are all brought up to it, our daily life is largely influenced by it, and our language has little difficulty in giving expression to it. All these reasons enable us to understand the powerful conservative properties of the mechanical view of nature. To transcend such a scheme of thought requires persistent mental effort and a prolonged self-discipline in 'philosophic doubt'. To understand both the success and the limitations of the existing scheme, to criticise it sympathetically and intelligently is extremely difficult. It is easy enough to brush it aside as 'abstract' and so to conclude that it is of no theoretical importance. It is much more difficult to understand how it can be true as well as abstract. But the student of science is given no instruction in these matters and the investigator is too busy as a rule to attend to them, even if he has the requisite inclination and ability.

Now turn to the subjective factors involved in this antithesis. First, there can be little doubt that in its dogmatic ontological form this antithesis is expressive of two profoundly antagonistic psychological types, and where such possibilities are unacknowledged and not allowed for there can be no possibility of a resolution of the conflict. But the introvert is not always a vitalist. He may take refuge in the view that the mechanistic standpoint, being a 'mere abstraction', although necessary for the pursuit of science, need not be

taken seriously in any ontological sense and can be completely ignored from the point of view of philosophy and religion. Next we have seen how very largely simple diversity of interest is responsible for this antithesis in its intolerant forms. We saw how differently the problem appeared to the physiologist, the embryologist, and the cytologist, and if we include other branches of biology in our survey we shall find still other divergences. Thus adherence to a particular point of view may be the result of nothing more than a failure to realize that the apparent simplicity of the problem is simply the outcome of limited knowledge or limited interest. A physiologist who is primarily interested and learned in the processes of digestion and proceeds to generalize over the whole field of biology on such a basis very easily comes to conclusions in which his own preferences have a larger share than the laws of nature. But this is to mistake what Kant called a *Wahrnehmungsurteil*, e.g. 'the vermouth is nauseous', which merely expresses a personal preference, for an *Erfahrungsurteil* which claims to be one to which all men will subscribe. It is simply an instance of the difference between the 'Ptolemaic' and the 'Copernican' points of view, and until people can be brought to understand that science is concerned to replace the former by the latter as far as possible, we shall continue to be plagued in biology with antitheses resulting from subjective influences of this kind.

We have also learnt how little the heuristic and the theoretical points of view are distinguished, except in the extreme form of calling the one science and the other philosophy. But heuristic success as such is no guarantee of truth, neither is it any guarantee that a given method will go on being successful indefinitely. Still less does it provide a *reason* for completely neglecting the study of other possibilities. But we have observed that the present problem is usually discussed by biologists simply from the standpoint of the investigator, as though laws of nature were simply to be picked up by the accumulation of data. This is in part a consequence of the divorce between the empirical and the theoretical points of view which followed the Cartesian dualism. This has not been so disastrous in physics on account of the mediating influence of pure mathematics, but in biology at the present day there is little recognition of the necessity for the rigorous thinking which the use of mathematics brings and demands,

and consequently there is little consciousness of what theoretical biology requires in this respect. But when once it is realized that thought is required to discriminate between heuristic success and truth, when it is understood that we cannot expect mathematicians and philosophers to do all our thinking for us, the importance of attending to 'principles of systematization' as a pre-requisite of theoretical biology will be better understood.

Among other factors which are responsible for the present impasse in regard to this antithesis may be mentioned various vague beliefs about the necessity of the demands involved in the mechanistic position for the bare possibility of science, coupled with the notion that anything else is 'mysterious' and that there is no other alternative. I have already pointed out that those demands were framed in the mechanical interest in the first place, and their use will, accordingly, only lead to a mechanical result. If they are used blindly and intolerantly we are again only solving the problem dogmatically in advance of unprejudiced inquiry. And with regard to the belief that mechanism is the only alternative to mysticism it is only necessary to recall the words of J. S. Mill that in scientific explanation we only substitute one mystery for another. The choice is not between mechanism and mystery, but between one mystery and another. It is simply a question whether the mysteries of physics can satisfy the requirements of theoretical biology, i.e. the systematization of biological knowledge.

Finally, as regards the question of the existence of other alternatives in addition to those offered by vitalism and materialistic mechanism it must be remembered that this is a question which may well require an extension of our intellectual horizon beyond that of the data of natural science. When this antithesis is discussed, especially by the upholders of mechanism and the machine theory, the discussion does not usually rise above, or burrow below, the level of naïve realism, or the modification of it which is the traditional attitude of physics. On such a basis there may well be no alternative, but what is the use of going on discussing such questions from such a standpoint? No one who reflects at all continues to defend either naïve realism or the physicist's alternative. But if this position is abandoned the scales fall from our eyes and a whole new horizon is opened up in which there may be

boundless unexplored possibilities. This is the truth which Dr. Haldane seems to have grasped and which other physiologists fail to understand and consequently are thereby prevented from appreciating. They suppose Dr. Haldane to be criticizing their methods of investigation whereas he is criticizing their metaphysical assumptions which they themselves are mistaking for naked reality. All this was discussed in Part I and need not be entered upon again in this place. But the suggestion is now made that by abandoning naïve realism and scientific materialism we may be in a better position to embrace those features of the organism, namely organization and development (both individual and racial) which present such difficulties for the materialistic view. And when I speak here of materialism I do not mean a metaphysical theory about the relation between mind and matter but a theory which believes the physical world or nature to consist of little hard lumps, too small to be visible to the naked eye, pushing each other about. It is this theory which is so difficult to reconcile with the notions of organization and development, and modern developments in physics.

There will be no occasion to abandon the old point of view so far as investigation is concerned provided it is not used dogmatically. But abandoning them as ultimate theoretical interpretations of the nature of the world of sense will enable us to make an honest attempt to look at biological facts more concretely, unencumbered by traditional theories. It will involve putting our pride in our pockets, acquiring the difficult virtue of intellectual humility and the equally difficult art of making an 'unusually resolute effort to think clearly'. What is wanted is more co-operation and less competition between rival theories. The notion that one theory excludes another and is to be regarded, therefore, as a competitor to be destroyed arises from our desire to regard our work as conclusive and exhaustive of what there is to know. But the simple consideration already urged that one system of abstractions cannot possibly from the nature of the case be exhaustive will show us how mistaken such an attitude is. We put the emphasis on the wrong place when we take our ambitions constructive schemes too seriously as a foundation to which everything must conform, when in truth they are only superstructures always liable to revision when increasing knowledge demands it. In biology our constructive theories have in

many cases been based on an intensive study of some limited field by one gifted and successful investigator who has then proceeded to generalise his results over the rest of the organic world. Under such conditions it is hardly surprising that there should be so much mutual rivalry and contradiction. But there has never been much effort made to widen the intellectual horizon of the biologist still further and challenge even the basal assumptions which this branch of natural science has shared with others. But, in the words of Professor Whitehead:

> 'The point before us is that this scientific field of thought is now, in the twentieth century, too narrow for the concrete facts which are before it for analysis. This is true even in physics, and is more especially urgent in the biological sciences.'[1]

[1] *Science and the Modern World*, p. 63.

CHAPTER VI

The Theory of Biological Explanation

A. *General*

We shall now leave these traditional controversies and try to consider the nature of explanation in general, and from this, putting on one side as far as possible our ordinary biological prepossessions, it will perhaps be possible to obtain a fresher view of the kinds of explanation which would appear to be required for the systematization of biological knowledge.

Apart from all more general or metaphysical questions the aim of explanation is to make clear the obscure, to render intelligible the unintelligible. What is experienced and demands explanation is obscure from complexity or from absence of relation to anything else. The process of explanation will consist, therefore, either in exhibiting the relation of what is to be explained to something else, or in diminishing its complexity by analysing it. Thus the methods of explanation involved in a science will depend upon (1) the entities to be explained; (2) the ways in which they can be analysed; (3) the relations holding between the entities to be explained and other entities of the same or of other kinds; (4) the relations holding between the entities into which what is to be explained is analysable.

Accordingly there appear to be two primary modes of explanation: (1) explanation by analysis, and (2) explanation by relating. But it is doubtful whether it is possible to speak of a purely analytical explanation, and in the actual procedure of science both modes are always operative. Disease was unintelligible until Pasteur showed its relation to the general phenomena of parasitism by discovering parasites by means of microscopical analysis. Dreams were unintelligible until Freud showed how the content and structure of a given

dream could be related to the past mental history of the dreamer as revealed by a psychological analysis. Even in the earlier days of chemistry when the procedure was purely analytical there was more than mere analysis involved in explanation, because the merit of the procedure rested not only on the result of the analysis but in the fact thereby revealed that there was a limited number of elements which entered into diverse otherwise unrelated bodies. Thus an enduring feature was discovered by analysis which could be generalized by means of the concept of composition. A theory of explanation, then, should have something to say about modes of analysis and about the nature of relations, and relational properties of various kinds.

Now since the primary data of natural science are the entities perceived by means of sense these entities will include whatever is spatio-temporally extended and is therefore analysable into spatio-temporal parts. But what is thus spatio-temporally extended is also qualitatively characterized. Thus the primary object of biological study—the living organism—is analysable into spatial parts which are characterised in various ways—e.g. in the various ways which are recognized as 'characters' in genetics. But there are many diverse modes of analysis. First there is what we may call *perceptual analysis*. When the geneticist analyses an organism into various characters he does not literally take it to pieces, he discriminates characters in it, usually by visual analysis, e.g. coat colour, and proceeds to investigate their mutual genetic relations as revealed in successive generations as a result of various crosses. He may speak of this procedure as a *genetical analysis*, of a certain limited part of the race of organisms in question. Next we have *manual analysis* yielding the entities revealed by anatomy. This is analysis of the organism into spatial parts considered as perceptual objects. It gives us organs, tissues, cells, and cell-parts as spatial parts in various spatial relations. In its extension by the use of the microscope it is largely based on visual experience although the use of the micro-dissector enables us to add tactual experience with a great increase in knowledge. *Physiological analysis* furnishes again another quite different type. Physiology does not consider the organism in abstraction from time but studies the mutual relations of the parts in the living organism considered as an event, yielding a complex

of mutually dependent processes. Finally *chemical analysis* yields parts in the relation of chemical composition, and if this analysis is pushed far enough it reveals entities which are discoverable also in the inorganic world. Thus these five methods of analysis yield very different results and a very little reflexion will show that the relations between them are by no means so sweetly simple and obvious as is commonly believed to be the case.

It is evident that explanation simply by analysis and comparison with other entities similarly analysed will not take us very far, and how far it will take us will depend on the degree of organization of that which is to be explained. In chemistry an analysis into *constituents* in fixed proportions has to be supplemented in organic chemistry by the additional notion of chemical *structure* in order to deal with stereo-isomerism, i.e. the bare notion of composition was not enough. In proportion as an entity has heterogeneous parts, and in proportion as those parts are so related as to exhibit a determinate spatio-temporal structure just so will a purely analytical interpretation be inadequate and require supplementation. Now the manual analysis of organisms shows them to be entities of this kind. They have a great diversity of parts which are analysable into smaller parts, these again into smaller parts and so on. But the parts of each level have perfectly determinate relations to one another, expressive of an elaborate organization. Biological explanations, therefore, will, so far as they concern the organism in abstraction from its environment, always involve far more than simple analysis but will have to deal with the complex relations between the various relata revealed by the analysis. Thus a theory of biological explanation requires a preliminary study of what we are to understand by organization, a question which, curiously enough, is rarely discussed in biological books.

So far we have been considering what appears to be involved in the process of explanation of a particular given entity but without reference to any existing body of knowledge. From the standpoint of any science, however, there will of course always be an existing body of knowledge containing generalizations, and the outcome of our analysis of a particular entity

may be to show that it is an instance of an already known generalization. But the degree of development of the science in question may be such that it contains very few generalizations, and the particular entity presented for explanation may not be embraced by any of them. Or the science may contain a great many unrelated generalizations. Such a science would be an ill systematized body of knowledge requiring improvement in many directions and at different levels of generality. There would be the necessity for discovering generalizations to embrace those facts which could not be accommodated by the existing ones, and there would also be required an investigation of existing generalizations with a view to discovering their mutual relations and possible reduction in number. Now generalizations signify the discovery of persistences of some kind, as explained in Chapter III, but in biology a great many persistences are easily discovered and the difficulty here is to analyse them and relate them to other persistences, and particularly to discover means of generalizing the different kinds of observed persistences. Instead of beginning with the data of a particular science and trying to discover means of generalization appropriate to deal with them we can borrow generalizations already established in another science. This has two advantages: first, it saves mental labour, and secondly it appeals to the intellectual desire for attaining the utmost generality, by offering the hope of showing that all the entities forming the data of a particular science may be represented as special instances of the generalizations of some other science. Both these processes have been going on in biology. We have discovered means of making biological generalizations and we have also discovered means of extending generalizations borrowed from other sciences—physics, chemistry and sociology. But in addition to all these generalizations, some confined to a particular science and others extending over two or more sciences, there are some extremely wide general notions common to all the natural sciences and constituting a kind of metaphysic of nature, namely the traditional materialism in the sense explained in the last chapter (p. 271). Consequently in discussing explanation from the standpoint of a given science we must bear in mind this vast existing system hovering in the background, and we can either take it for granted and conduct our discussion with this background as an acknowledged

basis, or we can try as far as possible to maintain a critical attitude towards it and approach the data of our science in an empirical fashion in order to see them in a fresh light. The latter alternative is the one I shall try to adopt although it is not an easy one to follow consistently.

Before we can turn to the study of the important question of organization a number of other topics require a brief consideration. Something should first be said here about the most important kinds of relations involved in the science of the organism. These appear to be as follows: (1) the relations holding between the organism and its environment; (2) the relations holding between an organism and other organisms, and these fall into two groups: either (a) when one organism is genetically related to another, or (b) when one organism constitutes part of the environment of another; (3) the relations holding between one part of an organism and another part of the same organism considered in abstraction from the rest; (4) the relations holding between the organism as a whole and a given part. It should be possible to state the essentials of biological knowledge with reference to such a scheme of fundamental relations and the relata which are found to stand in them, without any theory about what an organism 'is' just as it is possible to state the results of physics without any metaphysical theory about what 'matter' is or what 'energy' is. This would be 'pure' biology. And the traditional subdivisions of the science have in fact followed roughly some such scheme. But the presentation of the subject has suffered from the existence of the traditional biological antitheses, and from being too much influenced by the outlook of the particular author. If such a presentation as the above could be combined with all that is best on the *critical* side of the various traditional views we should be much better able to assess the precise status of the traditional controversial theories, and to see what concepts were required for the purposes of further generalization.

Now we should find in studying and expressing these various relations that some were of a kind which hold also between other entities than biological ones. But we should also find that some, although of universal occurrence among biological entities, do not hold between the objects of the inanimate world. For example, there are some objects (which may themselves be organisms) which stand in the relation of

'serving as food for' to organisms, and we find an analogy to this in some machines. Also every organism stands in the relation of 'descendent of' to other organisms. Again, one organism may stand in the relation of 'mate of' to another organism. We do not appear to find any analogy to this among machines. But these are characteristic biological relations, and if we are in too great a hurry to assimilate our knowledge of organisms to the rest of our knowledge of nature we shall be in danger of overlooking them. Moreover this danger is increased by the fact that some of these relations are familiar in everyday life, and familiarity always tends to draw a veil over our eyes which renders the most obvious things the most difficult to observe. We shall see that these questions are of great importance when we come to organisa-

The notion of 'intelligibility' appears to be involved in explanation and it will not therefore be amiss to examine this notion a little, especially in view of the way in which it is apt to be loosely used and so confused with other notions. The essential points to be considered can best be conveyed by means of an imaginary example. Suppose a man were whirled away to some other planet where everything is quite different from ours. Let him be permitted to remain there long enough to become familiar with his utterly strange surroundings and then be whirled back again to earth. It seems clear that on the terms of the hypothesis he would be quite unable to communicate his experiences. If these have been utterly different from earthly experience there will be no possibility whatever of conveying to earth-dwellers what he has witnessed, because there will be no possibility of any comparison—no point of contact between the two. Alice was able to record her adventures through the looking glass because they were, comparatively speaking, only a little different.

Thus every assertion we make about anything must involve reference to something else in the experience of the hearer, if the latter is to understand it. If I say snow is white to a man born blind I convey no information because white has no meaning for him. Consequently all propositions of a

science will be assertions about some aspect of its subject matter in terms of something else in the experience of the hearer if they are to be understood. If I say organisms are machines, or organisms behave in accordance with purposes, then these statements only convey information to people who have had experience of machines and purposes. Accordingly intelligibility is relative to the amount or variety of the experience of the person to whom the information is conveyed. In the words of Mr. Bertrand Russell: 'Every proposition which we can understand must be composed wholly of constituents with which we are acquainted.'[1]

Now intelligibility clearly has nothing to do with the truth. An assertion may be completely intelligible in the above sense but it need not for that reason be true. And a proposition may be true and yet, to a given hearer, quite unintelligible. But the term 'unintelligible' is not infrequently used when what appears to be meant is 'incredible.' Thus the assertion that the moon is made of cheese is perfectly intelligible to anyone acquainted with the terms 'moon' and 'cheese' and with the notion of 'making'. All that is meant in a case of this kind is that the proposition is contradicted by the rest of the hearer's knowledge and accordingly fails to generate belief; but if it were unintelligible in the strict sense it would not be contradicted. To a child it might be both intelligible and credible because he might have no other beliefs about the heavenly bodies with which it would conflict.

Then over and above being acquainted with the constituents of a proposition there is also 'seeing the connexion' between them, i.e. apprehending what is asserted by the proposition, and this process of apprehension of what is asserted depends partly on the degree to which we analyse the proposition. Thus 'snow is white' seems to be perfectly intelligible, credible, and in some sense true. It is perfectly intelligible to common-sense thought because common sense never troubles itself about the precise meanings of snow or white or about the nature of predication. But as soon as we do undertake such

[1] *The Problems of Philosophy*, London, 1912, p. 91. Another way of stating this is given by Mr. Russell in *Mysticism and Logic*, p. 211. 'Whenever a relation of supposing or judging occurs, the terms to which the supposing or judging mind is related by the relation of supposing or judging must be terms with which the mind in question is acquainted.' The original is in italics.

analysis the precise meaning of this proposition—what exactly it asserts—is by no means so obvious. That, then, is what I am referring to when I say that the intelligibility of a proposition depends not only upon acquaintance with its terms and the apprehension of what as a whole it asserts but also on the degree to which it is analysed.

But 'snow is white' is a proposition expressive of a judgment of perception, which is also the case with many anatomical propositions. Some of our biological knowledge, however, is expressed in terms of entities which are not perceptual objects but are pure concepts not *representable in imagination*. This brings us back to a question which has already been discussed in the Introduction and in Part I.[1] We noted that if we pursue the analysis of a natural entity a stage is soon reached beyond which manual and microscopical analysis cannot be continued, and recourse is then made to imperceptible entities. All that need be added here are some further remarks on the conditions governing the postulation of such entities. It is first important to bear in mind that since such imperceptible entities are invoked to *explain* something which *is* perceived they cannot be logically prior to the latter. They are therefore hypothetical and nothing can be asserted about them beyond what is required for the purpose in view—the explanation of the observed phenomena, and that means bringing the latter into some relation with other observed phenomena. If any such assertions are made they cannot be called scientific unless they are of such a nature as will admit of testing by a further appeal to perception. Moreover no such assertions can be made which are in contradiction to well ascertained facts in the same region of observation. If such hypothetical entities are already employed and well established in a given science their application to another sphere must not conflict with their older and better established usage unless there is good reason to believe that the latter is false.[2] It is evident that the above conditions impose certain limits to our making ontological assertions about these hypothetical entities, in addition to the limitations imposed by the fact that they *are* hypothetical. They should only be treated in conceptual terms because, as I have already pointed out, by thinking of them in terms of images we may

[1] See above, pp. 77-82, and Chap. II, Sections 7 and 8.
[2] An example of the breach of this rule is given below, pp. 369-8.

fall into the error of ascribing properties or qualities to them which conflict with the principles mentioned above. We only know such entities 'by description' not by 'acquaintance' and consequently the subject of any proposition about them will be a definition or description founded upon the data with which we are acquainted and which were involved in their first postulation.[1] Accordingly their intelligibility also will rest upon their first defining description.

With the aid of such concepts we hope to achieve generalizations and their value from this point of view depends on their high degree of abstractness not upon any spurious intelligibility they may possess in virtue of their being picturable in the imagination. But there is a powerful tendency to forget their conceptual nature and to treat such entities as 'more real' or 'more fundamental' than what is given in perception. This easily leads to all manner of errors. A good example of this tendency in natural science is seen in the following passage from Buckley's *History of Physics*, p. 242:

> 'Physicists have in general completely accepted the Quantum Theory, but it is realised that the theory does not explain phenomena at all, but merely allows us to describe them. Indeed, the conception has grown in many quarters, if we may quote Sir Ernest Rutherford, who does not, however, identify himself with this view, that "it may be quite impossible in the nature of things to form that detailed picture in space and time of successive events that we have been accustomed to consider as so important a part of a complete theory. The atom is naturally the most fundamental structure presented to us. Its properties must explain the properties of all more complicated structures including matter in bulk, but we may not, therefore, be justified in expecting that its processes can be explained in terms of concepts derived entirely from a study of molar properties. The atomic properties involved may be so fundamental that a complete understanding may be denied us. It is early yet to be pessimistic on the problem for we may hope that our difficulties may one day be resolved by further discoveries." Be that as it may, the position is not of the happiest.'

This passage is extremely interesting in connexion with our present topic. It illustrates the attitude of physicists in the face of the breakdown of the traditional type of explanation by means of a 'detailed picture in space and time' which has grown so familiar and has been so successful as to be regarded as *the* explanation *par excellence* as contrasted with a 'mere

[1] See Bertrand Russell, *Mysticism and Logic*, pp. 225 *et seq.* *Intro. to Math. Philos.*, pp. 167-180. Also *Principia Mathematica*, pp. 66-8 (and ed.).

description'. There seems to be a failure to understand the dual nature of such explanations, namely the mathematical part and the 'picturable' or imaginable part. So long as both are present the theory is regarded as an explanation, but if the picturableness is absent it is considered to be a mere description. Then the wording of the above passage suggests that the atom is something prior in knowledge and 'more fundamental' than the perceived molar properties of things which it was originally intended to explain. But in both cases what the atom is to explain and what the Quantum Theory is to explain is some perceived occurrence. If the atom gives us 'complete understanding'—which may be doubted—why, considering their epistemological footing, should not the Quantum Theory in its own sphere give us an understanding just as complete? The situation would appear to be one for congratulation rather than pessimism because it gives physicists a new field to explore demanding new concepts, and this does not at all detract from the dignity of the old ones. All that is meant by understanding in the above passage is understanding in the old familiar terms. And those physicists who oppose new concepts which are not reducible to the old seem to be in much the same position as those biologists who insist on rejecting biological concepts which cannot be reduced to those of some other science.

The properties of matter in bulk (by which is meant the perceptual objects from which physics and chemistry start) are explained by the properties of the atom in the sense that they 'follow from' the latter. But the former properties came first in knowledge and the latter have been postulated to *explain* the former. We only discover the 'properties of the atom' by searching for what they *ought* to be in order to explain, in the above sense, the perceptible ones of 'matter in bulk'. Now this procedure involves the assumption that as 'matter' is subdivided the increasingly smaller and smaller particles are governed by the same laws as the grosser visible ones. This suffices for the kinetic theory of gases. But chemical theory requires that when a certain stage is reached further subdivision yields entities with quite different properties. But the physicists continue to treat these further particles in the same way, and there does not seem to be any occasion for surprise if difficulties should now begin to make their appearance. As I have already tried to explain, the

trouble arises out of the fact that the possibility of using the imagination disappears at the stage now reached, but the mathematical treatment which has been doing the real work of generalization all along is still possible in the Quantum Theory and the physicist has to accustom himself to a purely conceptual explanation.[1]

This brings us to a task which has been deferred from a previous section—the analysis namely of the notion of chemical composition. We may take as our text for this purpose the following passage from William James:

> 'Readers brought up on popular science may think that the molecular structure of things is their real essence in an absolute s, and that water is H-O-H more deeply and truly than it is solvent of sugar, or a slaker of thirst. Not a whit! It is *all* of these things with equal reality, and the only reason why *for the chemist* it is H-O-H primarily, and only secondarily other things, is that *for his purpose of deduction and computation de-finitive* the H-O-H aspect of it is the more useful one to bear in mind."[2]

This view is, I think, true, and it is possible to subscribe to the above passage without in the least subscribing also to the typical pragmatist's attitude to science. The chief pitfall to be avoided here is the ambiguity of the word 'is'. When we say 'water is a solvent of sugar' or 'water is a slaker of thirst' we mean that these are among the properties of water. We do not mean that water is identical with any of these properties. But when we say 'water is H-O-H' we are then apt to think that 'is' here means 'identical with' and this is the belief against which James is protesting, and I think quite rightly. The situation is not so simple as that, and in order to understand it we require to bear in mind our previous analysis of the notion of property and to understand the nature of abstraction.

When we say 'water is H-O-H' we ordinarily think of this as expressing the *composition* of water, and we think of the properties as being in some way dependent upon and, if only we knew how, deducible from, this composition. But we knew the properties long before we knew the composition,

[1] Cf. the quotation from Boyle given above on p. 165.
[2] *Principles of Psychology*, II. 334 (foot-note).

We reached our knowledge of the latter by exploring many more of the properties of water than are encountered in daily life. Its solvent power and its ability to slake thirst did not lead to this notion of composition. The fact which does lead to this notion is the fact that under certain circumstances water ceases to be recognizable as water. It is then interpreted as having been decomposed into two entities neither of which is water. For, when water is boiled and the steam is passed over red-hot iron filings, what issues from the tube is no longer steam, and what remains in the tube is no longer iron. But by treating what is left in various ways iron can be obtained again, and something else besides, and if this latter something is mixed with what issued from the tube and a spark is passed through the mixture water once more appears. This then is typical of the kind of data which led originally to the notion of composition. Now clearly this perceptible disappearance of water in the presence of hot iron is as much a property of water as its ability to slake thirst or to dissolve sugar, and there seems to be no just reason why we should not speak of its appearance when oxygen and hydrogen disappear at the passage of a spark as also a property of water. These are properties of a peculiar sort in as much as they involve an abrupt change in the characters of what is characterized. We saw in Part I (p. 178) that to say that a certain thing of this kind has a certain property involves : (1) that it is known to belong to a certain class of things ; and (2) that when it comes into certain definite relations to certain other things it, or the other things, will exhibit a certain recognizable i.e. recurrent or pervasive type of change. The type of change we are now considering is one in which the thing disappears as such and two different things make their appearance. But one property remains constant throughout, namely weight, consequently the water is thought of as having these two things packed up in it in some way which conceals them, and as being pulled to pieces in the process in which this property is exhibited. Thought of in this way the disappearance of the water and iron and the appearance of hydrogen and oxide of iron in its place seem to be less mysterious. We substitute for the perceptible things which appear and disappear imperceptible things which do not themselves change, but only undergo reshufflings which 'account for' the 'apparent' changes. But when we add to these advantages the quantitative aspect of the fully developed

atomic theory, and remember further that the reactions mentioned do not stand in isolation but form only a small part—a few lines and nodes—of a vast web of observable and, by means of the theory predictable, changes which extends even to the stars, we see how difficult it is to escape from the belief that water is H-O-H and that this expresses its 'real essence in an absolute sense', and that it is *because* of this that it has such and such properties. Moreover it may be urged that *if* we knew more about oxygen and hydrogen we should be able to deduce all their properties on combination—not only the 'chemical' properties of water, but the thirst quenching and dissolving properties as well. But is there not something circular about this argument? What came first, what we really and indubitably *know about* water are its various properties, i.e. the persistences it manifests amid the flux of events. What came second and was inferred was the hypothetical formula for its composition. And this is really a mode of generalizing certain of these observed persistences, those namely for which it was originally devised and are distinguished as chemical. And the formula being thus devised to explain such properties, i.e. to relate one occurrence with another, can we say that the properties are deducible from and dependent upon this composition without being guilty of a *petitio principii*? Let us put the argument into a symbolic form, going back to the case of the water and iron:

(A-in-presence-of-B) yields X and Y, but Y under certain conditions yields (B and Z), and (Z-in-presence-of-X) under certain conditions becomes A hence A is regarded as (X and Z).

Here A stands for water, B for red-hot filings, X for hydrogen, Y for the oxide of iron formed, and Z for oxygen. Now the value of this whole procedure does not lie simply in its expressing in a condensed form some of the properties of water, but, as already said, in the way in which it occupies a determinate position in a wide scheme of generalizations, as is expressed, for example, in the notion of oxide. This notion links water in our scientific thought to a host of other entities which all exhibit a certain type of change in relation to oxygen. But this does not alter the fact that this scheme only subsumes properties of certain restricted types, and therefore does not embrace the solvent and thirst quenching properties of water nor its remarkable behaviour near the freezing point. These

are only dealt with by other schemes of generalisation. It is so easy to forget that in our experiment with the iron we have left out of account many features—e.g. the heat—which we have simply treated as conditions requisite for bringing certain changes about, because we have meanwhile been concentrating on those changes and constituents of the situation which are embraced by the concept of composition. Consequently it is easy to forget how abstract we are being. In predicating properties of water we think of them as 'inhering' in the water, i.e. as 'potentialities' standing to some substance in a simple two-termed relation, whereas the truth is that the properties on any given occurrence of their manifestation are actually properties of all the constituents involved and not merely of that which is known as the water, and the latter is only one term in a many-termed relation. Consider, for example, a crystal dissolving in water: from the 'standpoint' of the crystal solubility in water is one of its properties, whilst from the standpoint of the water the 'power to dissolve' the crystal is one of its properties. Now the molecular formula of water is a concise way of stating those of the properties of water which are especially related to the fact that under certain circumstances that which is known as water exhibits the abrupt type of change which we call chemical decomposition, and in which other entities (so regarded because they have different properties) make their appearance. Those of the properties of water which are not expressible in this way are embraced by the modes of generalisation of physics, physical chemistry or other sciences, or they may be for the present irreducible brute facts which fit into no scheme. It is the belief that the point of view of one science is in some sense 'deeper' or 'truer' or 'nearer to reality' than another that James seems to be protesting against in the passage quoted. But what is actually meant by these expressions is that different sciences differ in their degree of generality and this, as a rule, is in proportion to their degree of abstractness.

Now an incautious use of the notion of chemical composition may easily lead to fallacies in biology. For if the employment of chemical explanatory entities involves abstraction from entities of a higher level of organisation than the chemical, then the use of chemical notions cannot be exhaustive for those higher levels. Suppose, for example, that we dig

up a piece of chalk on the Downs and having given it to a chemist ask him what it is. He will say it is mostly calcium carbonate with certain 'impurities'. The notion of 'impurities' is highly characteristic of the chemical standpoint. The chemist works with an ideal in mind called a 'chemically pure substance,' and anything which may be left over is put into the category of 'impurities'. Similarly the physicist works with an ideal mathematical law and any deviations from this are called 'errors'. But now our lump of chalk 'is' a great deal more than calcium carbonate with impurities, and to learn about this 'more' we have to consult, not a chemist, but a geologist and a biologist. This further information is quite undiscoverable by the methods of chemistry and quite inexpressible in terms of its concepts. If chemists had not neglected such aspects of chalk, etc., there would have been no chemistry, and its failure to take account of them reflects no discredit upon it. Chemistry must for its purposes leave a great deal out of account, but that is not to say that what is left out is of no importance or is not 'real'. Neither does there seem to be any reason whatever for regarding what the chemist has to say as any more 'scientific' than what the geologist or the biologist may tell us. Nevertheless these considerations, simple and obvious as they seem to be, appear to be very commonly forgotten when the relation of chemistry to biology is under discussion. Just as, in dealing with the chalk, the chemist has to abstract from any determinate organization it may possess above the chemical level, in just the same way he must, in dealing with the living organism, abstract from any organization it may possess above the chemical level. The biochemist deals with abstract entities above the inorganic level but below the biological level. He deals with molecules which are themselves abstractions even on the inorganic level, and the organism is never simply a mixture of molecules, but these are constituents relatively low down in a complex hierarchy of organized parts, e.g. chromosomes, Golgi bodies, etc., which are characteristic of the species, i.e. capable of persisting in time, just as inorganic molecules at lower levels are capable of persisting. But the molecules with which the biochemist deals do not exist at all 'in nature'. They are always either constituents of a biological hierarchy or they are 'artifacts' i.e. made by human beings. Now just

as molecules (even if we take them quite naively) have different properties from their atoms, so do the organized parts of the organism which are self-propagating have characteristic properties which can only be discovered by studying such parts themselves, not only by studying their constituent molecules. Moreover, these parts—chromosomes, etc.—are themselves incapable of independent existance but are subordinate parts of a larger organic whole. From the point of view of 'independent existence' there is a great and real gap between the inorganic and biological realms. This is further shown by the absence of abiogenesis. The continuation of a given biological hierarcy is wholly dependent on the preservation of its characteristic organization, and it appears to be this organization which is involved in the synthesis of inorganic entities into biological parts. Thus the chemical method so long as it *is* chemical in the strict sense does not deal with the levels of the hierarchy to which the predicate 'living' is usually attached. It has created a new science—biochemistry—which is not inorganic because it deals with things not found 'wild' in the inorganic world, and not biological because these things only occur in the living body as non-living *parts* organized in a particular way. It thus deals with an abstract realm between the inorganic world and the living organism, and only if what is thus abstract is mistaken for concreteness will this point of view lead to fallacies. Now I take it that what Dr. Haldane did in his study of respiration was to try to investigate the abstract chemical aspects of the process as they occur embedded in their relations to the rest of the organism, and, according to his own account, his success largely depended on this point of view being borne in mind. This examination of the relation of chemical notions to biology has already brought us once more to the concept of organization which must now receive a more detailed and independent analysis.

B. Organization.

5

Anyone reading Professor E. B. Wilson's great book on the cell will hardly fail to be struck with the way in which the author, whenever he reaches a point at which, as he says,

'existing mechanistic explanations are inadequate' to deal with some observed cytological fact, falls back on the concept of 'organization'.[1] But he never discusses what precisely is meant by this expression in spite of the fact that he appears to consider it of great importance, but says that 'we are unable to define precisely the meaning of this vague term' (p. 1115). Similarly Starling, in the Introduction to his *Principles of Human Physiology* (p. 7, edit. 1912) writes:

> 'This short summary of the chief characteristics of living beings would be incomplete without the mention of what is perhaps their distinctive feature, namely, organization.'

But he, like Wilson, although apparently regarding it as something of great importance, only mentions it casually in the above way, and makes no attempt to discuss what it means, or to say why it is to be regarded as 'perhaps the chief characteristic of living beings'. And if this is the state of affairs it is hardly surprising that there should be so much fruitless and unending controversy about biological problems in which this important concept may be involved. Some notion of what Prof. Wilson has in mind may be gathered from the following passage which was quoted in part in Chapter V., but which deserves to be quoted in full:

> 'The fact of importance to the cytologist is that we cannot hope to comprehend the activities of the living cell by analysis merely of its chemical composition, or even of its molecular structure alone. . . . Modern investigation has, however, brought ever-increasing recognition of the fact that the cell is an *organic system*, and one in which we must recognize some kind of ordered structure or *organization*. The necessity for such a postulate . . . has not been set aside by conceptions of the cell as a colloidal system, or by modern investigations in bio-chemistry.'[2]

Prof. Wilson evidently thinks of this organization as being involved in structure as ordinarily understood, and on p. 635 he speaks of the 'structure of the chemical machine'. He also quotes a number of other authors who have emphasized the importance of organization:

[1] Cf. pp. 635, 670, 1006, 1035, 1037, 1067, 1114-7, in 3rd edits.

[2] Op. cit., p. 670. And yet, in spite of this, Prof. Wilson says on p. 1114 that 'heredity' in 'one of its primary aspects' 'has become a problem of biochemistry' and yet on the same page confesses that 'We are still without adequate understanding of the physiological relations between nucleus and cytoplasm and of the manner in which the nucleus is concerned in the operations of constructive metabolism, of growth and repair, and in the determination of hereditary traits.'

> Bütschli (1901): 'We must therefore ascribe to living cells beyond the molecular structure of the organic compounds that they contain, still another structure of different type of complication; and it is this which we call by the name of organization.'
>
> Jost (1907): 'One cannot help assuming that the mode of arrangement of the ultimate parts of the organism is of greater importance than the chemical nature of those parts.'
>
> Hopkins (1913): 'It is clear that the living cell as we know it is not a mass of matter composed of live molecules, but a highly differentiated system.'
>
> Mathews (1915): "The orderliness of the chemical reactions (in the cell) is due to the cell-structure, and for the phenomena of life to persist in their entirety that structure must be preserved.'

All this represents a clear recognition that organization above the chemical level is of great importance in biology, and yet in several parts of his book Prof. Wilson seems to lose sight of this fact and to contradict these statements. He says, for example, that:

> 'Nevertheless the only path available towards its exploration lies in the mechanistic assumption that somehow the organization of the germ-cell must be traceable to the physico-chemical properties of its component substances and the specific configurations which they may assume.'[1]

This of course is somewhat vague and might be so interpreted as not to imply a contradiction. In another place he says:

> 'It is our scientific habit of thought to regard the operation of any specific system as determined primarily by its specific physico-chemical compositions.'[2]

An intelligent outsider, reading these passages, would, I think, be astonished to find biologists disputing in this way about organization—to find them speaking of its having to be 'postulated', 'recognized', 'assumed', 'ascribed', etc. Is it not the first *fact* which strikes us about organisms? Is it not a bare analytical judgment (in the Kantian sense) to say that organisms are organized? Is not organization the very back-bone of the concept organism? And yet here are learned men telling us that we have to *assume* that the mode of arrangement of the parts of organisms is of greater importance than the chemical nature of these parts, that we must *ascribe* to living cells a structure beyond the molecular structure of the organic compounds which they contain. And then, finally, we are told that, in spite of all this, we must

[1] *Loc. cit.*, p. 1037. [2] *Ibid.*, p. 1115.

conform to a *habit* of thought which *assumes* that it must all be traceable to the physico-chemical properties of the component substances into which it is chemically analysable. How is this to be reconciled with the former assertion that the necessity of such a 'postulate' as organization has not been set aside by conceptions of the cell as a colloidal system, or by modern investigations in bio-chemistry? If the concept of organization is of such importance as it appears to be it is something of a scandal that biologists have not yet begun to take it seriously but should have to confess that we have no adequate conception of it. The first duty of the biologist would seem to be to try and make clear this important concept. Some bio-chemists and physiologists who write articles for popular consumption express themselves as though they really believed that if they concocted a mixture with the same chemical composition as what they call 'protoplasm' it would proceed to 'come to life'. This is the kind of nonsense which results from forgetting or being ignorant of organization, and against which the authors quoted by Prof. Wilson so clearly protest. Probably the whole difficulty is traceable to that 'scientific habit of thought' of which Prof. Wilson speaks. Habits are devices for saving mental labour not substitutes for it. All habits are not good habits, nor must we mistake them for laws of nature. The failure to take organization seriously is perhaps but another consequence of the rapid development of physics and chemistry as compared with the other sciences, and the consequent dazzling effect this has had on biological vision—making us blind to what lies most clearly open to our view, and leading us to look too eagerly past this to a hypothetical beyond. This has largely been responsible for our present scientific *habits* of thought.

It is a curious fact that organization is being taken more seriously in the physical sciences than in biology. One might almost go so far as to say that the concept of the organism has been discovered through modern physics. Biologists, in their haste to become physicists have been neglecting their business and trying to treat the organism not as an organism but as an aggregate. And in doing so they may have been good chemists but they have not been good biologists, because they have been abstracting from what is essential to the biological level. As Prof. Whitehead expresses it:

> 'The disadvantage of exclusive attention to a group of abstractions, however well founded, is that, by the nature of the case, you have abstracted from the remainder of things. In so far as the excluded things are important in your experience, your modes of thought are not fitted to deal with them.'[1]

If we are to make use of physics in biology let it at least be modern physics and not the warmed up corpse of Victorian speculation. Modern speculative physics takes organization very seriously in its dealings with the atom, and a good example of the importance of this notion is furnished by modern crystallography. It is clear that a purely chemical analytical study of a crystal is inadequate. To say that a crystal of copper sulphate consists of $CuSO_4$ is no explanation of its crystalline structure. It has been necessary to wait for a totally different method of analysis—the method of X-ray analysis. This has shown, for example, that the crystal-unit of quartz is not a molecule but a system of three molecules of silicon dioxide arranged in a special fashion having a certain screw-like character. I should express this by saying that the crystal has an organization above the chemical level. But crystals are but one type of a large class of entities having an organization above the chemical level which includes living organisms, machines, and works of art. That is why crystals, machines and works of art have all been employed from time to time as offering analogies with living organisms. Thus the machine theory of the organism tacitly acknowledges an organization above the chemical level and in this respect is superior to the 'physico-chemical' theory. Two principal factors appear to have been responsible for the failure to take organization seriously in biology. First there is the vague belief that only atoms and molecules are 'real', and secondly the incautious use of the notion of chemical composition. The entities studied by physics and chemistry are in some sense composite and some of their explanations are in terms of the relata in this relation of composition. Also the biological entities are found to be composite in the same sense, and moreover, some of the relata in the relation of composition in the physico-chemical objects are also relata in the biological objects. But the analysis of organisms as carried out by biologists reveals *other* relata in mutual relation of composition in a different sense, i.e. not in *chemical* composition, e.g. the organism is analysable into organ-systems,

[1] *Science and the Modern World*, p. 73.

organs, tissues, cells and cell-parts. There is a hierarchy of composing parts or relata in a hierarchy of organizing relations. These relations and relata can only be studied at their own levels (cf. the quartz crystal) and not simply in terms of the lower levels since these levels do not constitute *wnd* relata.

Now a potent factor in the present difficulty and one related to the above has been an improper use of the notion of *protoplasm*. This concept plays an important part in biological discussions, and biologists certainly speak as if there were in nature a stuff to which the term protoplasm is given. But when we come to look into the question there does not seem to be any justification for this belief. By this I mean we do not find any such stuff in nature in the sense in which we find water in nature. This may sound shocking but it seems to be the sober truth. Let us try not to be shocked at the sober truth but let us think a little. What we find in nature as distinct from the realm of knowledge are individual living organisms. These are analysable into a hierarchy of parts. Let us stop for a moment at cells. These are sometimes said to *consist of* protoplasm. This is a relic of the early days when it was really believed that a cell was a little drop of homogeneous jelly, but there is no excuse to-day for going on talking as if this were true when everyone knows it is false. If by protoplasm you mean a more or less homogeneous stuff in the sense in which a lump of putty is a more or less homogeneous stuff, in which there is no organization above the chemical level, then there certainly is no such stuff to be found in nature. What we do find in that which is known as a cell is, as Prof. Wilson says, a highly elaborated system of parts organized in a definite way. Moreover a given part e.g. the nucleus is itself possessed of an elaborate organization, and if the chromosome theory of 'heredity' has any truth in it each part of each chromosome will have a highly specific organization. If we turn to the rest of the cell we shall fare no better. We still find formed parts e.g. Golgi bodies and mitochondria, and as for the rest there seems to be every reason to believe, as the authors quoted by Wilson point out, that this too has a determinate organization above the chemical level. But if this is true we do not find protoplasm in nature. But there is another sense in which the term may be used. A biochemist may take a quantity of cells and grind them up with sand in a mortar and he may apply the term to the

ground up mass. This is a perfectly legitimate procedure, but clearly no such mass is found as such in nature either 'wild' or in living organisms. If a large bomb is dropped upon a populous town we might apply the term 'town-plasm' to the debris which remained, but it would be a little absurd to say that towns were *composed* of such town-plasm, and that from a sufficient knowledge of such debris it would be possible to gain an adequate knowledge of the organization of towns. But it is not more absurd than to talk as people do about protoplasm in spite of all the knowledge that has accumulated since that term was first introduced. The biochemist, if he is to study the parts of living organisms from a purely chemical standpoint, must, it seems, not only ignore any organization above the chemical level, but he must also destroy it in order to apply his methods, unless he confines himself to such fluids as blood and urine which can be withdrawn from the body. The information he obtains is of the utmost importance and interest, but it is information confined to the chemical level of organization. It would be unfair to expect it to transcend those limits for it would not then be chemistry, and it would be absurd to pretend that it has no limits, for every method of abstraction has limits. It is in virtue of those limits that it is a method of abstraction and is valuable. All this is, of course, perfectly familiar even to the elementary student, but it seems to be successfully forgotten by popular or polemical writers.

But no sooner have we cleared our thoughts a little about protoplasm than the whole difficulty breaks out again in regard to cells. Just as some people insist upon talking as though the possibilities of organization in nature had been exhausted at the level of molecules, so others seem to believe that these possibilities came to an end with the attainment of cells. This is another persistent relic of the days of the 'cell-theory'. We are told that the cell is to the biologist as atoms and molecules are to the chemist: an aphorism as misleading as it is crude. We are told that the organism is built up of cells as a house is built of bricks: a palpable untruth, since an organism is an organism from the start (if it has a start) whereas the house is not a house until it is finished. Organisms are not '*made*' they do not even 'develop'—something else develops as we shall see. Organisms merely persist—for a time. Finally, when it is said that

the so-called multicellular organism is an *aggregate* of cells this means, if it means anything, that there is no organization above the level of the cell, which is manifestly ridiculous.[1] Probably many biologists would admit this at the present day although a recent cytological writer considered it necessary to say :

> 'Nowadays, however, opinion tends in the opposite direction—to regard the organism as the individual, with a common life running through it all, and the cells not as units of which it is built but rather as parts into which it is divided in order to provide for the necessary division of labour involved in so complex a process as life.'[2]

Learned men seem to be prone to a peculiar kind of affliction which leads them to maintain all manner of impossible positions which have many years later to be recanted. The doctrine of preformation and the belief that there are no sexes in plants are two good examples from the seventeenth and eighteenth centuries.

Some references were made to the cell concept in Chapter II. (p. 158) and it is necessary to enlarge upon these in this place in order to remove some misconceptions which have survived from the cell-theory. It was pointed out in Chapter II that the mistake had been commonly made of treating the visual perceptual object in the usual naïve realistic manner as concrete, i.e. of confusing it with that which is known as a cell, when in fact, being a perceptual object, it is already an abstractum. But the cell *concept* is of course more abstract still—the term being defined as meaning a mass of cytoplasm containing a nucleus. The word cytoplasm can be understood here simply in a topographical sense as meaning whatever is left after the removal of the nucleus. Now there is of course no such entity as this to be found in nature. What is found in nature is a certain recurrent mode of organization amid the flux of events. And it is to this important fact that the cell concept gives expression. A protozoon, a fertilized ovum, an unstriated muscle fibre, and a spermatozoon all

[1] Even one of the co-founders of the cell-theory knew better than this, for he wrote that the cells : '. . . stehen aber nicht als ein blosses Aggregate nebeneinander, sondern wirken auf eine uns unbekannte Weise in der Art zusammen, dass daraus ein harmonisches Ganze entsteht.' Th. Schwann, *Mikroskopische Untersuchungen*, Herausg. v. F. Hitzeler, Leipzig, 1910. This seems to have been forgotten in the subsequent enthusiasm.

[2] Doncaster, *Cytology*, Cambridge, 1920, p. 3.

exhibit this mode of organization but they are very different entities and to confuse them is disastrous from the standpoint of theoretical biology. By a cell therefore I shall understand *a certain type of biological organization* not a concrete entity. A protozoon is a *whole* organism which is characterized throughout its history by the cell-type of organization. A fertilized ovum is a *temporal* part of an organism which latter is *not* characterized by the cell-type of organization *throughout* its history. An unstriated muscle fibre is a *spatial* part of an organism and this part is characterized by the cell-type of organization. And the spermatozoon is a special part, characterized by the same type of organization, which has been separated ' for the purposes ' of reproduction. It is as we shall see, a serious misdemeanour from the standpoint of theoretical biology to confuse a part with a whole. A so-called unicellular organism is clearly one in which the *whole* is characterized by the cell-type of organization, whereas a multicellular organism is one which has *parts* so characterized.

We now have to consider what is meant by the assertion that ' the cell is the unit of life '. In regard to any proposition there are always two questions to be asked: first, what does it mean? and secondly, is it true? It is often very difficult to answer the second question, and it is often difficult enough to answer the first, but it is quite impossible even to hope to find an answer to the second if we do not know the answer to the first. And yet people will frequently be found devoting large parts of their lives to discussing whether a proposition is true while resolutely refusing to spend even half an hour on the necessary preliminary question regarding what precisely it is they are arguing about.[1] In considering the meaning of the assertion that ' the cell is the unit of life ' a great deal depends upon what is meant by ' unit ' and by ' life '. Happily it does not seem necessary to stop at the word ' life ' because this term can be eliminated from the scientific vocabulary since it is an indefinable abstraction and we can get along perfectly well with ' living organism ' which is an entity which can be ' speculatively demonstrated ' i.e. pointed out. We only need to consider for the present what is meant by unit. In one sense it seems to mean any one thing which can be discriminated in perception from other things. Thus we should speak of a drop of

[1] Cf. p. 236 above.

water hanging from a pipette as a unit in this sense, which simply means a "substance" in Aristotle's primary meaning.[1] But if the drop falls into a vessel of water we could then only speak of the whole mass of fluid as a unit. But we could set up a standard of water-measurement, call it a unit, and find how many times this was exemplified in the whole mass. Suppose there were found to be 50 such units then we have evidently departed from our first sense because these units cannot be discriminated in the mass of water as separate drops, although the mass can be regarded *as if* it were composed of 50 such units. This illustrates the mathematical sense of the term, in which we abstract from everything but oneness—we arbitrarily choose a certain quantity as one unit and the total can be expressed as a multiple of this, i.e. as an aggregate of such units. If we took a heap of marbles we could regard either the whole heap as a unit or a single marble, according to our purpose in dealing with it. But the marbles may be of different sizes, colours, etc. If our purpose is to express merely the total number in the heap it will be sufficient to regard each marble as a unit; if we want to determine the number of a given colour then each one of a given colour will be the unit and so on. Hence what is called a unit in such cases is obviously dependent upon the purpose in view in relation to the complex, and a complete description of the heap would require a combination of an enormous number of different points of view. We should also have to take into account the shape of the whole and the mode of distribution of the different marbles.

In what sense, then, is the term unit used in the proposition: The cell is the unit of life? If by unit we mean a whole individual organism then clearly this proposition will be true of the so-called unicellular organisms. But if by unit we mean a part which can be discriminated in a whole—like the marbles of a certain colour in a heap—then the proposition will only be true of the cells in a given tissue. For example, in this sense an unstriated muscle fibre is a unit of a whole mass of smooth muscle but it is not a unit of the body as a whole since there are many different kinds of cells. But the proposition may also mean that what is meant by the concept cell, i.e. the cell type of organization, characterises the parts or certain parts into which the organism can be analysed. In

[1] Cf. p. 190 above.

other words some organisms are such that they can be analysed into parts all of which are characterized by the cell-type of organization, and this is true and important as long as it is not forgotten that (1) there are parts having an organization above the level of the cell-type, and (2) there are parts which are not cell-parts, and not analysable into cells, although these are not living. Probably it is in this sense that the above proposition is usually intended to be understood. But it is not true that an organism can be exhaustively analysed into entities called cells of which it is an aggregate, since this overlooks the two points referred to in the above sentence the first of which involves reference to the *relations* between the cells and *these are commonly overlooked*.

It seems clear that one idea which is essential to our notion of organization is that of one individual enduring thing having parts, and if this is the case it must be a spatio-temporal entity since only spaces and times have parts.[1] Secondly it is implied that the organization results from the mutual relations, both spatial and temporal, of those parts. The simplest kind of organized entity would be one in which the parts were all similar, only differing from one another numerically as far as their intrinsic characters were concerned, the organization being the result of their mutual relations. If the parts were homogeneous then we should be able to call them units and there would be only one level of organization. But if each part were itself composed of parts forming in each an organized system, then clearly we should have two levels of organization and if the composing sub-parts of the first parts were intrinsically only numerically different we could speak of two homogeneous levels of organization. If, however, the sub-parts were intrinsically different then the first-order parts would be different and we should have a heterogeneous type of organization. By carrying this process of subdivision further we could obtain very complex types of organization exhibiting a hierarchy of successive levels. And if now we consider one of the higher living organisms it is evident at once that its organization will belong to one of these heterogeneous hierarchical types. We have, presumably, the various inorganic 'organisms' i.e. organized entities—atoms, molecules, etc., which for all we know may have different properties

[1] We also speak of the organization of knowledge, in which 'some' and 'all' take the place of 'part' and 'whole'.

in virtue of the fact that they are members of a living hierarchy from the properties they exhibit in the inorganic world. In any case we should remember how abstract they are in their own sciences and we should be on our guard against taking them too naïvely and thinking of them in terms of perceptual models. Next we have cell-parts organized in a complex way. There probably are other levels between them and the known molecules of proteins, etc., but this of course is more hypothetical. There is a sufficiently complex organization in the perceptible cell. Above the level of the parts known as cells, i.e. characterized by the cell-type of organization we have *cellular parts*, i.e. parts *whose parts* have the cell-type of organization, and these are diversified in various ways according to the differences in the parts having the cell-type of organization and their mutual relations. But we have so far been thinking chiefly of the visual *spatial* structure and there is an urgent necessity for a consideration of *temporal* relations from the point of view of their importance for biological organization. Moreover there is that mutual relation of the parts in virtue of which what happens in one part may be dependent upon what happens in another part. Before we can discuss organization further we must consider time in greater detail than we have done hitherto.

Considering its importance it is surprising how little attention has been paid to time by biologists. Professor Alexander has remarked that 'it is Mr. Bergson in our day who has been the first philosopher to take time seriously', and since biologists in general seem to have a rather low opinion of Mr. Bergson it is not perhaps surprising that they have not yet learnt to take time seriously. But time is being taken seriously in the physical sciences, and if biology is to be in the fashion it will have to consider whether time had not better be taken seriously from its point of view too. Taking time seriously may have important consequences for biology as it has already done in the physical sciences. In Part I some suggestions have already been made towards taking time seriously. In what follows these suggestions will be expanded and explained in more detail.

No one has any difficulty in recognising that some objects

require a certain minimal space to manifest themselves in. For example, a piece of chalk can be cut up into smaller and smaller pieces which are still pieces of chalk. But chemists believe that there is a minimum spatial extension beyond which we could no longer have pieces of chalk. Now it has been pointed out that the same is true of the time in which some objects manifest themselves. To take an example from speculative physics: if an atom consists of a central nucleus with an electron rotating round it and you divide the time occupied by its history into a shorter period than one rotation you clearly have not given the atom sufficient time in which to manifest itself. It is not the kind of thing that could exist 'at an instant'. Now the same is clearly true of the living organism, in respect both to space and time. What we call a frog, for example, is evidently quite comparable with the atom. Considered apart from its history or temporal extension it is an abstraction. You cannot separate it in reality from its history. A frog without a life-history is as impossible as a life-history without a frog. A frog in pickle is a cross section of its history as a living organism, and anatomy is biology with the time-dimension omitted. Anatomy studies the organism in 'timeless space'. The following passage from Dr. Broad will help to emphasize this point:

> 'In ordinary life we distinguish between an object and its history, and we are inclined to think that the former is logically prior to the latter. We say, e.g. that there is a certain object, such as a penny, and that it may either rest or move, keep bright or tarnish, and so on. These events, we say, "happen to" the object, and its history is just all the events that happen to it. You might, we think, have an object without a history, but you could not have a history without an object. I believe this to be a profound mistake, which arises from taking "history" in too narrow a sense. An object separated from its history, is clearly not the kind of thing that could possibly exist. Every object that is not merely momentary has a history of some kind, and no merely momentary object could really exist. "Object", apart from "history", is therefore as much an abstraction as "history" apart from "object". Of course some histories are very tame, e.g. that of a penny which keeps in one place and never varies in its other qualities. Others are more exciting. . . . Now we are inclined to identify history with exciting, i.e. variable history. We then identify the object with the tame facts of its history; and forget that these are history at all, because they are so uniform. But really all that literally exists is strands of history, some tamer and some more exciting.'[1]

[1] *Scientific Thought*, London, 1923, p. 406.

> 'It is evident that every object has a time-dimension as well as any space-dimensions that it may have. There is nothing mysterious about this; it means no more than that every existing object, whether at rest or in motion, is a strand of history with some duration."[1]

It was the above passages which first brought home to me the importance of time in biology, but obviously those passages are not very satisfactory as they stand on account of the difficulties attending the term 'object'. On the view expounded in Part I objects are purely intellectual entities belonging to the realm of knowledge. They are what certain histories are *known as*. The object does not 'have' a history since it is non-temporal. What we find in nature are just strands of history with their qualitative characterization and mutual relations. Those which have an enduring mode of characterization are knowable as objects. It is on account of the intellectual nature of objects that they are more easily thought of than the histories of which they express the enduring characteristics, and it is for this reason that we think of the objects themselves as the histories and as momentary, thus depriving the history of its temporal character. The object is most easily discovered by concentrating on the tame tracts of the history and that is another reason why we come to confuse object and event. Objects do exist in the realm of knowledge and histories (with their qualities) alone 'literally exist' in the realm of nature.

There is still a further complication. Modern thought requires that space and time should not be separated but assimilated to one another, whence arises the concept of the four-dimensional event.[2] This does not mean as is sometimes supposed that time is 'a fourth dimension of space' but simply that space and time are both derived from a four-dimensional continuum from which they are abstracted. An event is a limited 'slab' of space-time. The living organism is such an event, and we saw in Part I that the perceptual object we also call the organism is expressive of

[1] *Ibid.*, p. 400. When later I came to read Prof. Whitehead's *Principles of Natural Knowledge*, I found that he had himself pointed the moral for biology on pp. 66, and 156.

[2] Cf. Part I, p. 181.

certain of the knowable characteristics of the event which can be exemplified in sense-experience. If we consider different slices across the time-dimension of an organism we find that its mode of characterization differs in different slices. The nature and degree of difference will depend upon (1) the time-interval separating the slices, and (2) what part of the total duration of the organism the slices are taken from. Broadly speaking we can distinguish (in Metazoa) two main (but partly overlapping) periods of the history: an earlier period—the developmental period, and a later period which we may call the 'behaviour' period. If our slices are taken during the developmental period they will exhibit greater differences of a certain kind than if they are taken during the behaviour period.

Thus in the organism successive temporal slices are different and these differences are serially ordered, i.e. not repeated in the same individual organism. Its very existence as one thing depends, as we saw (p. 199) on the continuation of a type of serial change. When this ceases 'life' ceases and the organism disintegrates into a mass of entities of lower organization. Thus it would seem that the notion of organization should also embrace this fact of intrinsic serial change. It should be noted that it is incorrect to speak of an ovum developing into a frog, it is a *temporal*[1] part of the history which

[1] The difference between temporal and spatial parts of events lies in the fact that only in the former is the character of the whole repeated. See Whitehead *Science and the Modern World*, pp. 190-1. 'It is in this endurance of pattern that time differentiates itself from space. The pattern is spatially now; and this temporal determination constitutes its relation to each partial event. For it is reproduced in this temporal succession of these spatial parts of its own life. . . . Enduring objects are significant of a differentiation of space from time in respect to the patterns ingredient within events. . . . It is not true that any part of the whole event will yield the same pattern as does the whole. For example, consider the total bodily pattern exhibited in the life of a human body during one minute. One of the thumbs during the same minute is part of the whole bodily event. But the pattern of this part is the pattern of the thumb, and is not the pattern of the whole body. Thus endurance requires a definite rule for obtaining the parts. . . . You must take the life of the whole body during any portion of that same minute. . . . The meaning of endurance presupposes a meaning for the lapse of time within the spatio-temporal continuum.' Compare also the following from *Principle of Relativity*, p. 68: 'The heterogeneity of time from space arises from the difference in the character of passage in time from that of passage in space. Passage is the mere *significance* and by significance I mean that

is the frog, or we can call it the character of that history during short early slices, and the adult is the character of later slices during which changes of a certain type are less rapid and obvious. Our fondness for thinking only of the adult as the frog is another example of our fondness for uniform objects. Thus to say that an organism develops means that it is temporally as well as spatially differentiated, and also that the temporal differentiation is serial, irreversible, or non-rhythmical, although spatial *parts* may exhibit rhythmical changes. And in this process of serial change during the developmental period the *spatial* organization becomes progressively more and more elaborate.

We now have to consider how this elaboration is accomplished. It is accomplished by the *repetition* of the original spatial organization. If this spatial organization were merely repeated in successive *temporal* parts we should simply have a uniform object persisting unchanged. But it is a fundamental and remarkable characteristic of living organisms—so familiar that we are apt to forget how remarkable it is—that they are capable of repeating their spatial organization *spatially*. So that from one exemplification of this spatial organization or pattern[1] there can arise by *division* two (or even more) simultaneously existing exemplifications, and these may have identical properties, i.e. they may, so far as we can discover, differ only numerically from one another. When this occurs experiment and observation show that there are often two possibilities: either the two exemplifications may remain in a certain organic relation to one another so as to constitute *parts* of one whole organism, or they may be separated (in a certain sense which need not mean spatially out of contact) in which case we have two whole organisms. In the first case we have a later temporal part (a later slice of the history) of the *same* organism which has, *ipso facto*, attained to a higher level of organization, since it now has two spatial *parts such* characterized by the spatial organization which previously characterized the whole. But in the second case we now have two whole organisms *such* exhibiting the same spatial pattern and each being of the same 'biological

[1] e.g. the fertilised ovum.

age ', which is another way of saying that they have the same biological properties.

Division may be an intrinsic or immanent process in the sense that it may not be directly correlated with any *external* event which can be called the ' cause ' of the division, in the sense, for example, in which a knife may be called the ' cause ' of the division of a loaf of bread. If we are to speak of causes we may of course say that penetration by a sperm is the cause of the division of the ovum, but only in the sense that it is one essential condition without which the internal changes do not usually occur. Additional divisions do not require further penetrations, and consequently it seems legitimate to speak of such divisions as occurring immanently or intrinsically, which is not intended to imply that they occur entirely independently of the surrounding medium. Now since this is a fundamental characteristic of living things it can, I think, be said that anything which can divide ' spontaneously ' or ' immanently ' in the above sense is alive, although the converse is not true in practice. A part may be alive and yet not be capable of division in the above way.

Now division implies something which is divided and what is divided is primarily space-time. From this it follows that only a certain slab of space-time of a certain minimum spatial extension can be the ' situation ' of an object of the kind called living. For below a certain minimum extension we reach entities according to chemical theory which cannot be divided and still retain their specific characters. There is a minimum extension, for example, below which chalk no longer exists. This is called the molecule of chalk. It follows, therefore, that what can be divided in the *biological* sense must have a greater extension than a molecule. Thus both spatially and temporally an event which is ' life-bearing ' requires a certain minimum extension. This enables us to understand why organisms got bigger in the course of progressive evolution. For to evolve in this case means (among other things) to differentiate, and to differentiate is to have more diverse parts, and this can be accomplished either by subdividing the already given extent or by increasing it. If the first alternative is taken we soon reach a stage where further division in the biological sense is impossible.

So far we have been considering division when the two products are what may be called *biologically equivalent*. This

happens when an ovum or a protozoon divides. In the case of many ova there are then two possibilities already mentioned: either the two products remain as parts of one organism, or in some cases each is one organism, e.g. in 'identical' twins. The former issues in development the latter in reproduction. We can speak of them as *part*- producing and *whole*- producing division respectively. In protozoa we have whole-producing division. But there is another kind of division which is whole-producing but in which the two wholes are not biologically equivalent. This is most simply exemplified when an animal lays a parthenogenetic egg, or when a bud is separated. Thus if an organism undergoes whole-producing division during the earliest slices of its history the two wholes are biologically equivalent, i.e. have the same specific properties or biological age. But if whole-producing division occurs late in the history the two wholes are of different biological age although in other respects they may have the same specific properties, and any differences they may exhibit may be the result of differences in their contingent environmental circumstances. This may also occur in the products of whole-producing division at the beginning of the history. The doctrine of the 'transmission of acquired characters' requires that when whole-producing division occurs late in the history of an organism the two products may be specifically different even although they would not have been different if division had occurred early in the history. Consequently such differences if they occurred would be correlated with the contingent environmental circumstances of the older organism.

The term 'biologically equivalent' is only applicable to the two products of whole-producing division because (as will be seen later) it is doubtful whether the first two blastomeres in a developing organism can ever be called biologically equivalent so long as they remain in an organic relation. Neither would the above remarks apply without modification to the division involved in sexual reproduction since the Mendelian doctrine requires divisions which yield parts that are not biologically equivalent.

In addition to 'spontaneous' or immanent division there is also division related more directly to transeunt relations with the environment, e.g. autotomy. This, however, is a further extension of the notion of division. Primarily I have used it to mean the spatial repetition of a type of organization, and

this is a property of living entities which exhibit the cell-type of organization. It may result either in two parts having this type of organization, or two wholes, or one part only may have the cell-type of organization. Finally we may extend it to such cases as budding in which neither part has the cell-type of organization but the bud is capable of repeating the mode of organization of the whole from which it is separated.

The nature of the relation between the products of division is important. They may be said to be in an *organic* relation (using the term in a technical sense) when they constitute parts of one organism, as in normal cleavage. They are then usually in perceptible contact. But the products may still be in contact and yet not be in an organic relation in the above sense, i.e. not related as parts of one organism. For example in the case of mammals the product of whole-producing division remains in physiological relation with the parent long after separation, when they have ceased to be in an organic relation in the above sense. Similarly a tape-worm may be within the body of another animal without being a part of it in the organic sense.

Symmetry—an important feature in biological organization—is the outcome of repetition. The term is usually confined to spatial repetition. But we can also have repetition in time and this is called rhythm. Biological organization involves both. In bilateral and radial symmetry the parts which are repeated to constitute the symmetry come into existence *simultaneously*. In spiral symmetry they come into existence *successively*. But there is another kind of repetitive division in which *pairs* of spatial parts arise successively and this is exemplified in metameric segmentation.[1]

Another fundamental concept required by biological organization can be called 'elaboration'. Elaboration is a process whereby something 'new' comes into existence. We must consider the different meanings of the term 'new'. In

[1] It is probably a misfortune that more attention has not been paid to symmetry along the lines of Prof. D'Arcy Thompson's *Growth and Form*. This should be extended from the four-dimensional point of view. A useful book on symmetry in general is F. M. Jaeger's *Lectures on the Principle of Symmetry*, Amsterdam, 1920. See also the chapters dealing with 4-dimensional geometry in Whitehead's books.

the first place a spatial part may be new in a given individual. In the course of his history a man acquires a nose which he did not have in the earlier strands of his history. But although this particular nose-part is new, noses are not new to the human race. Secondly, as we have just seen, when an ovum divides it acquires two new parts, and with the coming into existence of the two parts new relations come into existence. There is a difference between these two cases. In the latter case we have division, i.e. spatial repetition of a pattern already existing as characterizing a given organism. But in the case of the nose that organ did not exist as characterising the man in question. Thus both parts are in some sense new but one arises by division and the other does not. But both occurrences are instances of what I mean by elaboration. Thus part-producing division is a type of elaboration. Thirdly a part may be *new to the race*, and this possibility is demanded by the theory of evolution. When this happens we shall have a third mode of elaboration. Finally successive temporal parts of the organism as one persisting entity are new. But this, on the view of time here taken is an aspect of the temporal passage of nature—the continual coming into being of 'newnesses' as time passes. This concerns all the events of nature and is *not* part of what I mean by elaboration. Thus by elaboration I mean any process in which new spatial parts or new modes of characterization come into existence in the course of the organism's history.

There appear to be a number of different modes of elaboration:

1. *Organic synthesis.* A mode of elaboration in which inorganic chemical entities combine to form a higher level of chemical organization *in living organisms*, e.g. starch formation in the green plant. This process or similar ones can be imitated to some degree in chemical laboratories, but it does not otherwise occur in nature so far as we know, i.e. there is no abiogenesis.

2. *Division* (part-producing). In this process two or more parts come into being which are numerically different while still retaining the characteristic intrinsic properties of the original. It primarily applies to organisms or parts having the cell-type of organization, but also applies to living cell-parts, e.g. chromosomes. It appears to be a mode of elaboration only exhibited by living organisms.

3. *Histological elaboration.* The appearance of cell-parts (above the chemical level) not previously present in a cell, or the taking on of a specific shape by a cell.

4. *Inter-cellular elaboration* or elaboration of *cellular* parts. By this I mean the coming into being of differences between *territories* of cells, or cellular parts or regions, e.g. the elaboration of germinal layers and of organ-rudiments from these. This involves *transcendence* of cells, of which the following is an example. Suppose a particular patch of pigment on an animal is 'hereditary' and usually has a certain specific shape, size, and relation to the whole organism. Its elaboration involves a large number of cells. But even if we assume that they are all derived by division from some one cell this will not suffice to account for the shape, size, or relations of the patch. These involve mutual relations between the cells, and between these cells and the rest of the organism, and cannot therefore be explained simply by 'postulating' 'properties' in the parent cell. Theories of the Weismann type all make the mistake of concentrating on the intrinsic properties of individual cells to the neglect of their mutul relations.

5. *Syngamy.* Another mode of elaboration, and again one which appears to be peculiar to living things, is the process known as syngamy. This appears to be a process which is the very opposite of division since two parts come together to form a new whole. It is not surprising that syngamy should have been interpreted as a process in which certain supposed consequences of division are corrected. Some writers have believed that many 'newnesses' are to be traced to syngamy.

10

A little attention should be given to the notions of part and whole from the point of view of their importance for biological organization. A part, if it is a *living* part, i.e. one capable of division, cannot exist *in nature* in an inorganic environment. *If* it persists under such conditions it is the product of a whole-producing division and therefore no longer a part. In this respect living organisms differ from machines as ordinarily understood in which the parts are capable of existence independently of the whole without themselves becoming wholes. There is no such thing to be found in nature

as 'living matter'—we always find whole living organisms. Living parts are never found 'wild' as such.[1] Another characteristic of a part is, that what it is is always dependent upon other parts. A part may be anything from an electron (which may be different when it is part of a living organism from what it is outside it) to a leg. But all parts may not be living in the sense of being capable of division, and some parts may be living and yet not be capable of division. We have a hierarchy of parts:

A. Non-biological parts (incapable of division).
  a. Inorganic—occurring as such apart from the organism, e.g. mineral salts.
  b. Organic parts: normally only existing as parts of an organism: proteins, carbo-hydrates, enzymes, hormones, etc.
B. Biological parts.
  a. Capable of part-producing division.
    (1) Cell-parts: chromosomes, 'genes,' Golgi-bodies, etc. (only existing as constituents of cells).
    (2) Many non-cellular parts (cells).
  b. Not capable of division in higher organisms.
    (1) Some non-cellular parts (e.g. nerve-cells).
    (2) Cellular parts. Organs, which consist of territories of cells having a determinate organization *inter se* and in which more than one type of cell is discernible.[2]
    (3) Organ-systems.

We now come to wholes. A whole is capable of independent existence, i.e. it does not stand in an organic relation as part to a wider whole (except in the case of social organisms), but it has an environment of course, which is an inorganic one and with which its relations are to a large extent contingent. The relations between the parts in an organic whole are not contingent, and this is another important fact connected with the notion of organization. If we compare an inorganic whole—such as a crystal—which also exhibits the hierarchical type of organization, with a living organism, the chief difference we notice is in the way in which the successively higher levels of the hierarchy are formed. In the crystal it is by aggregation. In the living organism it is by division (but not entirely as we have seen, since there are other modes of elaboration), and this may be expressive of a diminution of contingency in the relations between the parts. But the most important

[1] The gametes form an exception to this.

[2] A tissue might be defined as a homogeneous cellular part, i.e. having cells all of the same type, but in actual fact most tissues are not homogeneous, e.g. muscle always has connective tissue in it.

feature of the mutual relations between the parts constituting a living organism is the fact that those relations are *internal* relations so that the properties of a part are different when it is in its place in the organic hierarchy from what they are when it is removed from it.[3] Examples of this have already been given and more will be brought forward below.

We require now to summarize the progress so far made in our attempt to discover what is involved in the important concept of 'organization'. In the first place it presupposes the notion of one individual thing having parts, which is distinguished from an aggregate by the nature of the relations between its parts. For this reason it was necessary to criticize the notion of 'protoplasm' which abstracts from the organization of the living organism, and is a survival from an earlier time before very much was known about cytology. Similarly and for the same reason it was necessary to criticize the notion that the so-called multicellular organisms are aggregates of cells. We have also seen that, in the case of living organisms, differentiation in time is as essential to the understanding of their organization as differentiation in space. A living organism is an event no two temporal parts of which exhibit the same characterization. Moreover organization of the vital level requires a certain minimum of both temporal and spatial extension. Its spatio-temporal differentiation is achieved in virtue of the fact that there is a certain type of organization in organisms which is capable of spatial repetition by division. This is the cell-type of organization. The real significance of the cell concept lies in the recognition of the fact that, apart from bacteria and the lowest known organisms of that primitive type, this is the simplest type of organization which is capable of characterizing a whole (and hence independent) organism, but in virtue of its spatial repeatability it also constitutes the type of organization which characterizes certain of the *parts* of those organisms whose organization is above this level. But there are also other processes in virtue of which the type of organization can be raised and these were also called processes of elaboration. Parts of one organism

[3] This obvious truth was, of course, pointed out by Aristotle, but has been neglected by the post-Cartesian tradition.

are organically related and this relation is such that the parts behave differently in this relation from what they do out of it. That is to say the parts of a living organism are internally related to one another, and such relations are not contingent as are the relations of the whole to its *inorganic* environment. Thus a living organism is analysable into a hierarchy of parts in a hierarchy of relations, but neither its parts nor their relations are unchanging since it is differentiated temporally as well as spatially. During the behaviour period, or earlier, some of the parts exhibit temporal differentiation of the rhythmical type.

12

It remains now to see a little further what is involved in saying that the mode of organization is 'hierarchical'. It is the tacit recognition of this character of biological organization which Prof. Wilson expresses when he says: 'We cannot comprehend the activities of the living cell by analysis merely of its chemical composition, or even of its molecular structure alone'. I said that this recognition seems to be recanted by Prof. Wilson in another place when he says: 'Somehow the organization of the germ-cell must be traceable to the physico-chemical properties of its component substances'. Let us conceive an entity having the hierarchical type of organization, and let it be analysable into four primary relata A, B, C and D, standing in certain organizing relations. Let each of these be composite so that A is analysable into the relata a', a'', a''' and a''''. B is similarly analysable into b', b'' . . . etc., and the same with C and D, and between the a's there will be organising relations and so on for the b's, etc. For the sake of simplicity we will suppose that the a's, b's, etc., are not further analysable. We then have two levels in our hierarchy. First there is that constituted by A, B, C and D in their relations and we can call this the X-level. But if we carry the analysis further we have another level constituted by the a's, b's, etc., which we can call the Y-level. We shall also have different types of relations according to the level under consideration. The relations between the relata of the X-level we can call Rx. When we come to the Y-level we shall have the relations between the a's, those between the b's, and so on. Consequently the kinds of

relations on the Y-level will equal in number the number of kinds of relata on this level, and we shall have Rya for the relations between the *a*'s, Ryb for those between the *b*'s, etc. Now the properties of A will be the outcome of the properties of the *a*'s *and their relations*. That is to say A is what it is not merely because it is composed of *a'*, *a''* . . . etc., but because of the organizing relations between *a'*, *a''*, etc. But if A is internally related to B this means that A and B have different properties according to whether they are in an Rx relation or not, just as two blastomeres may behave differently when they are in an organic relation from what they do when they are separated. Now it seems to be perfectly clear that if you analyse the entity under consideration down to the Y-level you may discover the properties of, say, the *a*'s or *b*'s, in isolation, but you discover nothing about the X-level because it has ceased to exist. To study the X-level we have to take the X-level as it is and investigate the mutual relations between the A, B, C and D relata which together constitute that level. We can discover how the whole behaves without A and how A behaves without the whole. We can also investigate A analytically in isolation from B, C and D, and so try to understand how the *a*'s and their relations constitute A, but we should have to remember the possibility that the *a*'s may behave differently when A is in the Rx relation from what they do when A is isolated.

Now let us apply this to the living organism. For the sake of simplicity let us suppose that cell-parts (p. 309) are ultimate although of course this is not the case. Then the various cell-parts described by cytologists will constitute with their several relations one level—the lowest—of our hierarchy; call it the Z-level. Then the various non-cellular parts ('cells') so constituted together with their relations will form the Y-level, and the cellular parts will form the next and highest level or X-level—the level into which we primarily analyse our organism.[1] Such a hypothetical organism would have no 'chemical' relata but we know that if the analysis is pushed further we do come to chemical relata and the chemist in order to reach them usually has to destroy all the intervening levels. Moreover it seems too obvious to require mention

[1] There are of course complex determinate relations between 'cellular parts', which latter do not constitute a homogeneous level. These are not considered in the above simplification. See footnote on p. 309.

that the relations between, say, a muscle cell and a cellular part e.g. a limb can only be studied in the limb itself—not to mention the fact that the limb is itself a part and its relations to the whole are important; for example, we tacitly take it for granted that the limb is supplied with oxygen which presupposes respiratory and other cellular parts.

If A, B, C, D, etc., constitute the cellular parts then their relations will be Rx (A, B, C, D, etc.) and they can only be studied at that level. If A is isolated from the rest you have destroyed the relations of the X-level at least so far as A is concerned. But if A can be kept alive by furnishing it with an artificial substitute for its organic relations you may be able to study its constituent relata in their relations, i.e. Ry (a', a'', a''', a'''', etc.) i.e. the cells and their relations which together constitute A, but only in so far as these are not altered by the fact that A is no longer in the relation Rx to B, C, D, etc. Then you can take a given cell a' and study it in isolation—e.g. in tissue culture, thus investigating its constituents in their relations Rz ($b'a'$, $b''a'$, $b'''a'$, etc.). But it must be remembered that when a part is isolated it no longer has its typical relations, in other words its environment is different, and the concept of environment applies as much to parts as to wholes, with the important difference that the environment of a part is the whole organism and not an inorganic environment. Hence when a part is isolated one of three possibilities will be realised. (1) If it is put into an inorganic environment it may behave as a new whole (e.g. an isolated piece of a planarian). (2) It may perish, i.e. cease to persist as a living thing as when a man's leg is cut off. (3) It may be furnished with an artificial organic environment, as in many physiological experiments, tissue-culture, etc. This process of isolation is absolutely indispensable in many cases in physiological research and the results obtained are of the highest interest—an enormous part of our knowledge is dependent upon them. But from the point of view of interpretation of these results it is extremely important to bear in mind one obvious methodological point: it is never safe to assume uncritically or assert dogmatically that an isolated relatum say A taken from the level Rx (A, B, C, D, etc.) exhibits the same properties in isolation as it does in its place in the level—even when it is furnished with an artificial organic environment which is as 'normal' as possible. For to assume this

is to assume as I have said that the relations symbolized by Rx are purely external relations making no difference to the terms standing in them, and this is never a safe assumption to make in biology. But it is an assumption which is almost invariably made by physiologists even without realizing that it is an assumption. We saw an example in the passage from Claude Bernard quoted on p. 242. This is simply the result of our fondness for things and our neglect of relations, and also of the preponderance of physical as contrasted with biological ways of thinking in daily life as well as in scientific laboratories. It is always legitimate to make the above assumption for purposes of investigation, and so long as it is remembered that it is an assumption when we come to the question of interpretation no harm is done. But difficulties arise simply because this never is remembered except by physiologists of the rare kind exemplified by Dr. J. S. Haldane.

Now all this is of enormous importance in medicine. For here we always have to do with the physiology of the organism as a whole in which, although the normal relations of the parts are disturbed, they are not disturbed by isolation as they are in physiological experiments. Let us take an actual example in order to make the importance of organization quite clear. A cell in the convoluted tubule of the kidney has at least the following relations: (1) it has other kidney cells in contact with it; (2) it has lymph, blood-capillaries and connective-tissue cells in relation to its proximal or basal end; (3) it has a fluid of varied chemical character in relation to its distal end (according to Cushny's theory). Now consider an isolated kidney-cell in tissue culture. How many of these relations are realized? At most we can say that some of the relations under (2) are in part realized in so far as the isolated cell is furnished with an artificial organic environment of constant chemical character based on a knowledge of the chemical character of the tissue fluids. Now the beautiful experiments of Dr. Drew show conclusively that the relations of the kidney cells to the connective tissue cells are *internal* not external, since the cultivated cells behave quite differently according to the presence or absence of connective tissue cells. Moreover the absence of relation (3) will mean that the kidney cell is not exhibiting its typical 'function'. It will, consequently, exhibit the basal metabolic changes common to all cells but not those characteristic of it *in situ*. Thus

valuable as is the information we shall be able to elicit from the cultivation of kidney cells in this way we cannot expect it to tell us *all* we require to know about such cells since it cannot tell us precisely what the kidney cell activities are when it takes its place in the hierarchy. The kidney cell has been shown to behave differently according as its relations are altered. Its full characteristics as a *kidney* cell are only manifested when it is in its place in the Y-level, and that again is related to the X-level. That is to say if Ry (k', k'', k''', k'''', etc.) represents the kidney, then k' (a kidney cell say from the convoluted tubule) will be quite a different cell in isolation from what it is when it stands in the relation Ry to the other cells (of various kinds) in the kidney. It is an abstractum not a relatum, although we go on *calling* it a 'kidney cell'. Moreover, even the kidney itself is what it is in virtue of its relations in the X-level, i.e. it is the K in Rx (A, B, C, D . . . K . . . etc.). That is to say we have a hierarchy of parts standing in multiple internal or constitutive relations to one another. And accordingly we require, for theoretical purposes, to bear this in mind when we come to interpret the results of a purely analytical investigation. It will be necessary to devise ways and means of obtaining data requisite for correcting a purely analytical procedure.

Now this example, based on empirical data, seems to me to show quite definitely that the assertions of those who say that biology must *only* use the concepts of physics and chemistry is not only not practised but not practicable. And when people say, as Mr. J. Needham says, that 'At the present day zoology has become comparative biochemistry and physiology biophysics', or, as Prof. Marshall says: 'Putting aside the aesthetic and historical interests of biology as extrascientific, and medicine, which is an art as well as a science, biological investigators in general find the categories of chemistry and physics to be sufficient for their own studies'—when people say things of this kind they seem to be talking sheer nonsense based on ignorance of the science as a whole, which they suppose to be no wider than the small branch in which they happen to be interested. Taken on their face value such statements are so obviously and grotesquely untrue that it is difficult to believe that their authors can possibly mean what they say, and biological writers are so much addicted to saying what they do not mean in order to convey what they *do* mean

that it is never possible to feel quite sure about what is intended. But from what has been said about organization it seems perfectly plain that an entity having the hierarchical type of organization such as we find in the organism requires investigation at all levels, and investigation of one level cannot replace the necessity for investigations of levels higher up in the hierarchy. And this remains true irrespective of the question of a remote future possibility of being able to state the properties of all the higher levels in terms of the relata in the lowest one, if indeed the very nature of the case does not exclude such a possibility. Apart from all such speculations the fact remains that a physiologist who wishes to study the physiology of the nervous system must have a level of organization above the cell-level to begin with. He must have at least the elements necessary to constitute a reflex arc and in actual practise he uses concepts appropriate to that level which are not the concepts of physics and chemistry. But there is no such thing as a nervous system 'up in the air' in isolation from other parts, and it would be quite impossible to study such a thing physiologically, and consequently we are compelled to admit more and more of the organic hierarchy. Physiologists persist in confusing isolation in thought with isolation in nature.

Moreover in discussing organization we have been forgetting two other important points which have their bearing on biological explanation. We have forgotten the relation of this organization to the contingent circumstances of its environment, which presupposes constant changes in it apart from its own intrinsic changes, and we have forgotten that this organization is believed to have evolved, and in that evolution its level of organization has been gradually raised. With each increase in organization new terms in new relations have been elaborated and it is sheer dogmatism to suppose that under such circumstances no new types of regularity or law have come into being. But if we shut our eyes to these considerations and refuse to think biologically we may be shutting ourselves off for ever from making any enlargement of our knowledge which such possible new types of law would require. Dr. Broad has expressed this truth when he says:

> 'If in fact there are new types of law at certain levels it is very desirable that we should honestly recognise the fact. And, if we take the mechanistic ideal too seriously, we shall be in danger of

ignoring or perverting awkward facts of this kind. This sort of over simplification has certainly happened in the past in biology and physiology under the guidance of the mechanistic ideal."[1]

This appears to me to be the best *scientific* attitude, if science is at all concerned with the truth and not with making everything fit into a pre-determined mould. Professor Pembrey gives a good example of the blinding influence of chemical dogmatizing :

> ' At one time the formation of fat from carbohydrate was contested on chemical grounds, although the positive evidence from the physiology of animals and plants was concordant and adequate. . . . It is impossible, and ever must be, to decide on the evidence of pure chemistry what is and what is not possible in the laboratories of the living organism. Questions of physiology must be decided on biological evidence, and progress is delayed when the biologist awaits the uncertain guidance of the chemist and physicist."[2]

This I entirely agree with because it is quite illegitimate to say what is or is not possible on a question of this kind in one realm of nature from knowledge based on another realm, especially when that knowledge is highly abstract. This particular example seems to be but another instance of that peculiar stupidity of the learned to which I have already referred (p. 295). Let me finally call in an open-minded physicist to witness to the same truth as the above authors have expressed. Professor P. W. Bridgman writes as follows on p. 2 of his *Logic of Modern Physics* :

> ' The first lesson of our recent experience with relativity is merely an intensification and emphasis of the lesson which all past experience has also taught, namely, that when experiment is pushed into new domains, we must be prepared for new facts, of an entirely different character from those of our former experience."

13

I will now summarize my reasons for believing that an *exclusive* attention to what are called mechanical explanations in biology is not desirable, bearing in mind that vitalism is not the only alternative, but that purely biological explanations are also possible :

[1] *The Mind and its Place in Nature*, p. 77.
[2] In article ' Physiology ' in *Evolution in the Light of Modern Knowledge*, London, 1923, p. 173.

1. *Depending on the relation between the stage of development of a science and the perfection of its means of observation.*

The history of physics shows a close correlation or adaptation between the theoretical interpretations of a given stage and the complexity of the data available at the time, the latter depending on the nature of the instruments for observation then available. That is to say the data were not too numerous or complex to be dealt with by the modes of thinking then elaborated. Each new step in improvement of instruments of precision has made possible a further advance on the previous stage, resulting in a further refinement of physical laws. Physics has progressed from crude laws or rough approximations of a low degree of generality to more precise laws of a high degree of generality. Poincaré has written:

> 'If Tycho had had instruments ten times as precise we would never have had a Kepler, or a Newton, or Astronomy. It is a misfortune for a science to be born too late, when the means of observation have become too perfect.'[1]

The moral of all this for biology is sufficiently obvious. Modern physics and chemistry place in our hands means of observation of great refinement, and means of interpretation of a highly sophisticated nature, very far removed from what can be directly perceived. As Mr. Russell puts it:

> 'Physics and perception are like two people on opposite sides of a brook which slowly widens as they walk; at first it is easy to jump across, but imperceptibly it grows more difficult, and at last a vast labour is required to get from one side to the other.'[2]

We are thus furnished with data of great abundance, and interpretations of great complexity, just as Kepler would have been had he possessed Michelson's interferometer. We are, therefore, in danger of being overwhelmed by our data and of being unable to deal with the simpler problems first and understand their connexion. The continual heaping up of data is worse than useless if interpretation does not keep pace with it. In biology this is all the more deplorable because it leads us to slur over what is characteristically biological in order to reach hypothetical 'causes'.

Another point connected with the above is the fact that such laws as have been so far discovered in physics are rela-

[1] *Science and Hypothesis*, Eng. trans., p. 181.
[2] *Analysis of Matter*, London, 1927, p. 137.

tively simple [1] but they have, nevertheless, required the most brilliant mathematical talent for their elucidation. Their relative simplicity has been one important condition of their discovery. It is not enough that there should be laws but they must be simple enough for us to be able to unravel them. But there is no reason to suppose that the laws governing the more remote relata in the biological hierarchies are simple, and if the only explanations which can be called scientific are those of which the laws of mathematical physics are the type then the outlook for scientific biology would not appear to be very bright. This is no reason for not trying to find such laws, but it is a reason why we should not cease to seek other means of systematizing biological knowledge.

2. *Depending on the abstract character of the mechanical explanations.*

As I have already repeatedly pointed out, the nature and existence of *abstraction* in all scientific procedure appears to be very easily forgotten. The whole success of the scientific method depends on abstraction and on dealing with problems piece-meal. But one consequence of this is that a given mode of abstraction cannot be exhaustive. To abstract means to discriminate certain features in thought from others which are meanwhile neglected. Any conclusions which may be reached will, consequently, only be valid under the conditions of the mode of abstraction employed.[2] The success of a given mode will depend on the skill with which relevant features are selected. But if there are other modes of abstraction they will also require exploration if there is reason to believe that they are important, in order to check the conclusions reached by the first. We must not let the success of one mode of abstraction be set up as an obstacle to the exploration of other ones. What does it mean to say that an organism is alive? Modern biology has no answer to this question because it is under the influence of one mode of abstraction which abstracts, and from its nature must abstract, from the fact that the organism is alive. An unprejudiced comparison between the living and the non-living might furnish data for an answer, but if we start from the non-living and attempt to work upwards—

[1] Cf. the passage from Mr. Bertrand Russell quoted above on p. 208.
[2] Cf. p. 292 above.

trying to piece all the levels of the organic hierarchy together from a knowledge of its lowest relata—we shut out any possibility of such a comparison. No genuine grounds have been brought forward for supposing that the hopes of dogmatic mechanism will continue to be upheld, any more than there were any grounds for supposing that Victorian physics would always be upheld in all regions of physical phenomena.

3. *Depending on the intrinsic limitations of the mechanical explanations.*

Biologists are apt to take a far too naïve attitude towards physics, interpreting it more realistically and dogmatically than physicists themselves, without any understanding of its history, of its metaphysical basis, or of the traditional difficulties which have been inherent in some of its concepts from the beginning. But, as everyone should know, these difficulties have become particularly acute at the present day and are the subject of much renewed controversy. It is a great mistake, therefore, to suppose that in building upon physics we are building upon an impregnable and unmoving rock. Professor Whitehead has said that:

> 'It cannot be too clearly understood that the various physical laws which appear to apply to the behaviour of atoms are not mutually consistent as at present formulated. The appeal to mechanism on behalf of biology was in its origin an appeal to the well attested self-consistent physical concepts as expressing the basis of natural phenomena. But at present there is no such system of concepts.'[1]

It is a good plan to get what aid from outside is available, but it is much safer and would appear to be the *primary* duty of each science which is entrusted with a given subject-matter to appeal always to the discoverable data which that subject-matter provides, and reject whatever is contradicted by it, rather than to try to make it accept notions borrowed from other spheres.

[1] *Science and the Modern World*, 2nd edit., p. 119. Cf. Weyl, *Raum, Zeit, Materie*, 1921, p. 283: 'It must, once and for all, be said quite clearly, that physics is no longer able to support in its present state, the belief in a closed causality of material nature resting on strictly exact laws.'

4. *Depending on the existence of hierarchies of levels of organisation in living organisms.*

This has already been discussed at length. Analysis destroys the characteristically vital level of organization. You can only deal with a given level in terms of the relata in that level. Such simple considerations are entirely overlooked by the majority of physiologists even although their actual practise is in contradiction to their theory. It is encouraging to find that this is becoming understood by the younger generation of physiologists. Dr. L. T. Hogben, for example, writes that the usual physiologists' programme, according to which their science consists of the study of the physics and chemistry of the organism:

> 'If carried out consistently . . . necessitates the elimination of all reference to some of the most characteristic properties which distinguish living systems. Generally speaking, those who restrict the scope of physiology to phenomena for which ready made physico-chemical explanations are at hand, make an exception for the treatment of reflex action. By some obscure convention this grace is rarely extended to the phenomena of reproduction.'[1]

Moreover the existence of the individual living thing, having a hierarchy of levels of organization, as the primary entity with which biology is concerned should warn us against being too easily content with purely atomistic notions, however great may be their value from the investigator's standpoint. A purely analytical explanation as contrasted with methods of investigation is obviously out of the question in theoretical biology. Regarding analysis Mr. Johnson writes:

> 'the important process is—not the mere revelation of the parts contained—but rather the indication of their mode of combination within the whole, analysis is better defined as the exhibition of a given object in the form of a synthesis of parts into a whole. . . . to analyse *X* simply means the same as to exhibit *X* as a synthesis.'[2]

Now this cannot possibly be done by working *solely* from below. It requires that we study each level in itself in terms appropriate to it as well as in terms of concepts appropriate to lower levels.

[1] L. Johnson, *Logic*, Part I, Cambridge, 1921, p. 107.

5. *Depending on the nature of the hypothetical method.*

Methodological mechanism in its various forms proceeds by borrowing explanatory principles which have been elaborated in one sphere and for one purpose and extending them hypothetically to a totally different one. It may therefore be described as a non-homogeneous use of hypothesis. Now from a logical point of view and therefore from the point of view of theoretical biology this is an extremely hazardous procedure, however successful it may have been in the accumulation of data. As an example of a hypothetical argument which has actually been used in biological literature and the incorrectness of which has been pointed out by a physiologist we may take the following:

> 'The force developed in a muscle-twitch diminishes with rise of temperature.'
>
> 'Surface tension causes diminution of force with a rise of temperature.'

Therefore:

> 'Muscular contraction must be due to surface tension.'

Professor A. V. Hill, commenting on this argument, does not remark on its logical weakness but calls attention to certain facts which invalidate it. He says:

> 'This argument is invalid. The relation of tension to heat-production in a twitch is *independent of temperature*, a given liberation of lactic acid produces, at all temperatures, the same rise of tension. The system, moreover, is certainly never in a state of reversible equilibrium; hence no such thermodynamic arguments can be applied.'[2]

But from the logical point of view the argument is equally assailable. It assumes that wherever there is 'diminution of force and rise of temperature' this diminution is 'caused by surface tension'. This assumption is borrowed from physical science and carried over bodily into the biological sphere. When a case in the latter sphere is then discovered of 'diminution of force with rise of temperature' it is concluded that this case also 'must be due to surface tension'. Of course there is no ground whatever for 'must', at most we can say 'may'. Only if we knew that *all* such diminution was 'due to surface tension' would the argument be valid, but as far as the realm of biology is concerned it is just these major

premises which remain to be discovered. The above argument is a good example of the fallacy of 'asserting the antecedent on the ground of the consequent' and is quite comparable with the following one given by Professor Hobhouse:[1]

> 'If at one time the climate of Europe was Arctic in character fossil remains of "Arctic" animals would be found.
> 'They are found.
> 'Therefore the climate of Europe was Arctic.'

'This form of argument', adds Professor Hobhouse, 'suits good and bad inferences equally well. . . . It gives neither certainty nor probability; it gives in strict logic nothing at all. . . . The hypothetical form of statement is one which the mind easily follows, and is therefore plausible.' Thus a hypothesis is not verified because facts can be picked out which are 'accounted for' by it. Its probability only becomes considerable when the phenomena it is able to embrace are both numerous and varied, so as to increase the probability of excluding other possible hypothesis. A hypothesis suggested by an intimate acquaintance with a wide range of biological facts is more likely to be near the truth than one imported from another sphere. This is what I mean by saying that the use of physico-chemical hypothesis is a 'non-homogeneous' use of hypothesis and therefore requires caution. It should be sufficiently obvious that by the hypothetical method in the above sense we introduce nothing new into our interpretations. We shall get nothing more out of our hypothetical procedure than what we put in. Thus, in physiology, if we frame purely physical or chemical hypotheses we can expect to get nothing but physical or chemical results, and if there are laws peculiar to entities of the vital level of organization we shall never reach them in this way. It is not at all surprising therefore that the 'progress of physiology' should continue to 'support' the claims of mechanism. The hypothetical method as followed in that science consists in taking some physical or chemical law or inventing a physical or chemical model, and seeing how far it will work in biological systems. This always yields valuable new data, as indeed any hypothesis would which suggested new experiments, but it usually proves in the process to be untenable and further investigations lead

[1] *Theory of Knowledge*, p. 409. The whole of Chap. XVII, on Induction and Hypothesis should be consulted in this connection.

to further complication of the original hypothesis or to its abandonment for another. Now this is not induction and cannot lead to the discovery of biological laws. Hypothesis leads to the discovery of facts and this encourages the belief that those who practise it are making progress. And so they are but in discovery not in interpretation. True induction on the other hand aims at finding from the facts of a given sphere the universals proper for their embodiment in thought. In hypothesis the story in general outline is assumed to be given in advance and it hopes that the facts will conform and fill in the detail.[1] From these considerations it is clear that it will be in the highest degree a precarious procedure to take the explanatory notions which have been inductively reached in one science and employ them dogmatically and uncritically in another. But this is just what we do in biology when we *exclude* all other methods of interpretation but those employed in the physical sciences. So far there has been extraordinarily little genuine induction in biology. Even Darwin's selection hypothesis was not a purely biological one. He borrowed ideas from sociology and from artificial breeding and then found that such biological data as were at his disposal could be accommodated to this hypothesis. But facts are very accommodating as we see from the way in which two rival hypotheses are able to make use of the same range of facts with totally contradictory results.

6.[2] *Depending on the fact that modern biology regards organisms as the outcome of an evolutionary process.*

We have not yet discussed the implications of evolution for biological explanations although some difficulties were noted in our examination of Jenkinson's opinions in the last chapter. But it may be pointed out here that the modern doctrine of emergent evolution is inconsistent both with dogmatic mechanism and with purely methodological mechanism if the latter wishes to exclude all other methods of interpretation. Consequently any one who subscribes to the doctrine of emergent evolution cannot at the same time subscribe to dogmatic mechanism nor to the exclusive adherence to methodological mechanism.[3] But in any case my arguments based on the

[1] Compare the example given above on p. 213.

[2] [illegible] to be added after Chap. X., p. 432.

[3] [illegible] of Prof. Lloyd Morgan quoted above on p. 110.

nature of the organic hierarchical mode of organization suffice to show the difficulties of limiting biological interpretation in the way attempted by methodological mechanism.

I think the whole situation can be summarized in the following way: If it is asked whether the concepts of physics and chemistry suffice for *present day* scientific biology both in investigation and interpretation the answer is no, because neurology, experimental embryology and genetics, to mention only the most outstanding branches which claim attention to-day, chiefly employ purely biological concepts although always with some kind of a mechanistic[1] speculative background. If the question is asked whether these concepts will *ever* be dispensed with and their places taken by purely physical or chemical ones then one obvious answer is: Wait and see. But a more interesting and less timid answer would be as follows: We could begin by asking a question: Are there any entities in the world which exhibit a mode of organization above the chemical level, and are living organisms included in such a class? If the answer is in the affirmative then we can say that probably the correct answer to the first question will be in the negative. If, however, our question is answered in the negative, then we have to ask: What then do you mean when you say (as you do say) that living things have evolved from the inorganic? If there is no organization above the chemical level what has happened in evolution? This question will arise again when we come to the antithesis between preformation and epigenesis.

Let me in conclusion say that the above arguments are in no sense directed against the intelligent and legitimate use of mechanistic explanations in biology, nor are they intended to discourage the attempt to extend them. They are intended simply to discover the conditions of biological explanation from the methodological standpoint, and to show *reasons* for the desirability of some biologists devoting their attention to developing a biological way of thinking as contrasted with the way of thinking employed by the physical sciences on the one hand, and the psychological method of interpretation which seems to underlie vitalistic thinking on the other. Neither do I wish to discredit the efforts of the vitalistic writers in so far at least as they attempt to deal with difficulties which their rivals are content to ignore.

[1] At least in the fourth sense distinguished on p. 299.

CHAPTER VII

The Antithesis between Structure and Function

Biological thought has been harassed by this antithesis throughout its history. Debates still go on about whether 'structure determines function' or 'function determines structure'. This antithesis manifests itself in the separation between anatomy and physiology, and, not infrequently, in quarrels between anatomists and physiologists. Physiologists claim that their study is more important than anatomy because function is 'more fundamental' than structure. And anatomists have been known to entertain the belief that with a sufficient knowledge of anatomy it would be possible to 'deduce' function. Some physiologists, on the other hand seem to take the opposite view from that just mentioned. Thus Starling, writing of the correlation between 'histological differentiation' and the 'increasing efficiency of adaptive reactions attained by the setting apart of special structures (organs) for the performance of definite functions' writes:

> 'This parallelism between the development of function and structure justifies us in the assumption generally, though often only tacitly, accepted by physiologists that the structure is the determining factor for the function.'[1]

Also we sometimes hear experimental embryologists saying what a good thing it would be if physiologists would help them to elucidate the 'functional' side of what they are studying. The following remarks of Professor E. B. Wilson on this antithesis are interesting:

> 'Whether structure or function is the primary determining factor in vital phenomena is a question that has been a subject of debate for many generations of biological philosophers. As thus studied, however, the question has proved barren, for all students of the problem have in the end had to admit that structure and function are inseparable. It is certain that vital action is not known to us apart from an organised tissue, and equally certain that vital structures exist only as products of protoplasmic activity. Thus has arisen a dilemma which belongs to the fundamental philosophy of biology and may have to be left aside as practically insoluble.'[2]

[1] *Principles of Human Physiology*, 1912, p. 9.
[2] *The Cell*, 3rd edit., New York, 1925, p. 670.

It seems then that this antithesis is still acute and that if it could be removed in some way a troublesome antithesis could be removed from biological thought. And this I think we are now able to do on the basis of our foregoing discussions. Far from being insoluble it is the easiest of all the antitheses to overcome. A little reflexion will show that it rests simply on the separation of space and time and if this separation is not made the antithesis between structure and function falls to the ground. I have already pointed out that anatomy abstracts from time. It studies the organism conceived 'at an instant'. Physiology, on the other hand, cannot do this. Evidently then, the first thing we have to do is to find out what precisely is meant by the vague terms structure and function. When we have done this it will I think be possible to understand this antithesis and to see how it can be overcome.

We shall begin with 'function'. It is clear even from the passages quoted that this term is used ambiguously. It appears to have three principal meanings which are quite commonly confused. (1) In the first place when we speak of the function of an organ we usually mean what rôle it plays in the economy of the whole. We frequently find that this means that the maintenance of some character of some other part is dependent upon the part or organ in question. Thus we say that the function of the heart is to keep the blood in motion, and the function of the kidney is to maintain a certain constant chemical character of the blood. We often speak of the function of a part of a machine in much the same sense. (2) In the second place we sometimes speak of certain 'fundamental vital functions' such as assimilation, respiration, excretion and so on as inseparable characteristics of living things or their living parts. These notions belong to the cell-concept when it is enlarged from the physiological standpoint. Thus what seems to be done in this case is that certain pervasive types of change are recognized as exhibited by entities having the cell-type of organization, and these discriminated types are called 'vital functions'. (3) Thirdly the name seems to be used as a name for all the processes ordinarily said to be 'going on in an organ'. This seems to be what is meant when we speak for example, of the functioning of the kidney,

or of the renal function. This is what physiologists usually study when they are said to be studying the physiology of the kidney. These three meanings seem to be tolerably clear and quite distinct from one another. It is with the third meaning that we shall here be concerned since it is function in this sense that is usually contrasted with structure.

We now have to consider what is meant by structure. What does an anatomist mean by the structure of the heart? It is at once evident that what is usually meant is *spatial* structure, and that what the anatomist deals with is an *object* not an event. What he usually studies is something much more abstract than the heart as it is in the living body (although anatomy is usually considered to be a highly 'concrete' science). The anatomist usually studies the pickled heart, i.e. the heart after it has ceased to 'have' a biological history which is an exciting history and difficult to observe, but only a tame history as a uniform physical object. Moreover he studies it chiefly as a visual object and owing to our preference for visual experience and our persistent naïve realism it is extremely easy to fall into the error of thinking of the visual heart as the very concrete heart itself. But of course the visual heart though 'real' is already an abstractum. The visual shape of the heart is a synthesis of many visual appearances of the heart as it appears after pickling. If microscopical sections are studied we have something more abstract still. A little bit of the heart has been subjected to a highly sophisticated technique. Moreover it has one of its spatial dimensions greatly reduced. Thus what is seen through the microscope is something greatly simplified and far removed from the concrete heart. Now consider the heart as it is in the living body during a concrete duration. It is intolerably complex, but it is still possible to know it as a persisting thing having the rhythmical type of change of characterisation. Which of all the fleeting appearances it presents are we to call *the* structure of the heart? Obviously the heart is an event (or a part-event of the event of greater spatial extent which is the organism) of the kind which is only knowable as a non-uniform object. Its characteristics are only exhibited over a period of one whole 'repeat' of its series of rhythmically

repeated changes. But even this is not strictly true because the living heart does not beat uniformly. One of its characteristics is its variability in relation to other events forming part-events of the same organism. Of course we have to simplify and abstract, and anatomy represents one way of doing this in regard to living organisms—one mode of abstraction. And physiology when it studies the isolated heart represents another. It is not in the least derogatory to anatomy and physiology that they abstract since without abstraction they would be impossible. But if, when we come to interpret their results synthetically, we forget their abstract nature, then we fall into the 'fallacy of misplaced concreteness' and innumerable avoidable 'antitheses' are the result.

If we remember what has been said about the doctrine of four-dimensional space-time events and their characters we see at once that the heart considered as 'something going on' is such a four-dimensional event, it is the heart as studied by physiology, i.e. the 'function' in our third sense. Now the perceptual heart as studied by anatomy, on the other hand, is an object characterizing that event. The living heart is knowable as a rhythmical non-uniform object. As such it is difficult to study. Anatomy therefore studies it when it has ceased to be living and treats its purely spatial aspect in abstraction from time. Thus we may say that anatomy deals primarily with the spatial aspect of those part-events of organisms whose characters are especially enduring. Moreover it deals especially (in microscopical anatomy almost exclusively) with their spatial aspects as visually apprehended. It is not difficult, therefore, to see that anatomy is and must be very abstract, and only if we mistake what anatomy teaches for the concrete four-dimensional event which is characterised, shall we get into inextricable difficulties over the relation between 'structure' and 'function'. There is no such antithesis in nature. The antithesis springs solely from our modes of apprehension and from the separation of space and time. Moreover it is obvious that 'structure' as ordinarily understood cannot be said to 'determine' 'function'. We can now interpret Professor Wilson's dilemma. He says:

> 'It is certain that vital action is not known to us apart from an organised basis, and equally certain that vital structures exist only as products of protoplasmic activity.'

This dilemma may belong to the 'fundamental philosophy of

biology ' but it need not for that reason be left aside as ' practically insoluble ' because that philosophy should help us to solve it and I think it does. All that we mean by vital action is the organism or part of the organism as an event amid other events. Such events, unlike, say, certain electro-magnetic waves, happen to be of the kind that are knowable in perception as having more or less enduring characters. We abstract the visual spatial aspect of this and called it the ' organized basis ' of the activity whereas it is only an aspect of it deprived of its temporal passage.[1] It is an empirical fact that such aspects are only observed in some connexion with such events, so that if we find them we expect that we are in the presence of such ' vital activity ' or we connect them in thought with such activity (e.g. in the case of fossils). In just the same way if we came across a gramophone in the Sahara desert we should believe that some man had put it there.

It seems, then, that what is required is an enlargement of our concept of ' structure ' so as to include and recognize that in the living organism it is not merely a question of spatial structure with an ' activity ' as something over against it, but that the concrete organism is a spatio-*temporal* structure and that this spatio-temporal structure is the activity itself. Moreover, as was pointed out in Chapter III, temporal differentiation is just as obvious and important a characteristic of the living organisms as is spatial differentiation.

It is beyond the scope of this book to work out the consequences of the resolution of this antithesis in detail in relation to particular biological problems. But we shall make use of this key in later chapters, and more will be said below (e.g. p. 440) about the nature of ' structure '.

[1] We are still left, of course, with the antithesis between event and ' object ' or character. See below, p. 376.

CHAPTER VIII

The Antithesis between Organism and Environment

From the standpoint of traditional chemistry and physics, or, more correctly, from the standpoint of the traditional scientific materialism in which those sciences have expressed their results, there is no antithesis between organism and environment. Both are resolved into swarms of particles in which—here and there—are momentary condensations. From such a standpoint most of the problems of biology disappear. The following passage from Professor Bridgman's *Logic of Modern Physics* (p. 35) will show how such a point of view

> 'If our experience had been restricted to phenomena in a vacuum, and the objects we were trying to count had been spheres of a gas which expand and interpenetrate, it is obvious that the concept of "object" as a thing with identity would have been much more difficult to form. Or if our objects are tumblers of water, we discover when our observation reaches a certain stage of refinement that the amount of water is continually changing by evaporation and condensation, and we are bothered by the question whether the object is still the same after it has waxed and waned. Coming to solids, we eventually discover that even solids evaporate, or condense gases on them, and we see that an object with identity is an abstraction corresponding to nothing in nature.'

This illustrates clearly the paradoxes we get into through that inversion of abstractions and concreteness which happens as a result of our habit of taking the provisional ultimates of science as 'more real' than the experience from which we started in order to reach them. The objects with identity which we perceive are certainly abstractions, otherwise they would be of no use to thought. But they express the enduring characters of events just as do the atoms of the physicist. Both are 'abstractions'. Thought can, it seems, only work in this way and the objects of physical science are enormously more abstract than those of daily life and for that reason are more valuable for scientific purposes. Both are implicated in nature as expressive of permanences in the characters of events. If

we deny this and take the objects of science as 'more real' or more concrete than the objects of perception we only get into the hopeless tangles which were discussed in Part I. Science calling the objects of perception abstractions 'corresponding to nothing in nature' is a case of the pot calling the kettle black. If they corresponded to nothing in nature common sense would never have discovered them, neither would physical science have come upon atoms if *they* 'correspond to nothing in nature'.

The theory of the organism as a perceptual object amid other objects, and their relations to events has been discussed in the preceding pages. From this it will be seen that in the present discussion the antithesis between organism and environment is accepted as a genuine and irremoveable one. An organism without an environment is never an object of experience and to a biologist it is unthinkable in the sense of unmeaning. Thus if we begin, as good emprical biology should begin, with what we observe, the organism in its environment constitutes the complex with which biology has to deal and which enters as we shall see into all problems connected with the remaining antitheses.

The relation between organism and environment does not appear to be so simple as is sometimes supposed. The organism considered as an event exhibiting a certain mode of characterization which is one of serial change presents a sharp contrast in its regularities to the shifting inconstancy of many of the contemporaneous environmental events. An environmental change may have any one of the following consequences from the standpoint of the organism:

(1) There may be no correlated change in the organism.
(2) A change may be exhibited by the organism which may belong to any one of the following types:
  (a) Changes resulting in dissolution and death
  (b) An enduring change in the typical mode of characterization of the organism as determined by a knowledge of the race to which it belongs.
  (c) Changes may occur which lead to the maintenance of its typical course (i.e. restitutive processes of all kinds).

Accordingly there does not appear to be a simple one-one correlation between organic event and environmental event. Moreover a departure from the mode of characterization typical of its race may be exhibited by an organism which is not *known* to be correlated with an environmental change, e.g. mutations.

These questions will arise in connexion with the remaining antitheses.

The three antitheses which remain are the most difficult and most unsatisfactory to deal with at present. But they cannot for that reason be ignored. Even if we can do no more than analyse them and expose their roots clearly, without being able to overcome them, this in itself will be something accomplished. It will at least have the merit of revealing the avoidable obscurities, of giving us a profounder insight into the difficulties of biological knowledge, and of making clear the problems which confront the biology of the future.

CHAPTER IX

The Antithesis between Preformation and Epigenesis

We now have to study one of the most important and most difficult antitheses involving, as it appears to do, a very large part of biological science. Professor E. B. Wilson, referring to the 'problem of Preformation and Epigenesis' in his great book on the cell, writes:

> 'Perhaps this problem is scientifically insoluble; at any rate no general agreement has yet been reached in regard to it. The modern biologist must be permitted to treat the problem in pragmatic fashion, employing the terms of one hypothesis or the other according to the procedure that he finds most useful in practice. . . . Weismann and Driesch . . . alike express the opinion that fundamentally epigenesis is inconceivable. . . . It means . . . that we are unable to conceive how a self-determining system can increase its own initial complexity by interaction of its chemical and physical components. In so far as such a system is independent of external causes it can only transform and redistribute components that are inherent in the system from the beginning.'[1]

Professor Wilson also adds that 'in their purely logical aspects the questions here raised belong perhaps rather to metaphysics than to science'. It seems, then, that we have difficulties to deal with of a particularly deep-seated kind, and if there is to be any 'general agreement' it will only be reached from a wide and detached point of view. I should agree that the 'pragmatic' attitude is the correct one for the investigator, but it is clearly the duty of the critical theoretical biologist not to be content with this. His first duty is to try to understand what precisely the problem involves. And this duty cannot be shirked by handing it over to 'metaphysics'.

We have first to ask, then, what is meant exactly by the terms 'preformation' and 'epigenesis' and what precisely is the problem with which these two concepts are concerned? And to answer these preliminary questions, to disentangle all

[1] *The Cell*, 2nd ed., New York, 1925, p. 1110.

the complex issues involved, is itself no easy task. It is one which is made all the more difficult by the usual habit of biologists of neglecting such necessary preliminaries at the outset of discussions of this kind. One thing seems tolerably clear: that the concepts of preformation and epigenesis have to do with *developmental processes* and consequently one of the first things to do will be to discover what precisely constitutes a developmental process. The doctrine of preformation, in its original meaning, signified that development is simply a process of progressive *increase in scale*. Whereas the theory of epigenesis, whatever else it may imply, asserts that development is *not* simply a question of increase of scale, but a process in which a genuine increase in 'complexity'[1] is progressively achieved.

If we refer back to our discussions of permanences or regularities in nature in Chapter III we see that development is a serial process manifested as changes in the mode of characterization of the successive temporal parts of an event knowable as one enduring thing, i.e. one individual organism. We know that it is a regularity or a recurring, and hence recognizable, mode of change by discovering that it characterizes a great number of similar things. But we also find that a given organism characterized by a given series of changes is related to one or more other organisms, also so characterized, by the genetic relation. The developmental process is thus a serial process which is repeated in a genetic succession of individuals, so that a given succession, considered as a continuous process, has a rhythmical character.

We may contrast this with the serial changes exhibited by a given crystal of a radium salt. The fact that this is a regularity is also discovered by finding that it characterizes a great number of other individual things of the same kind. But these individual things do not stand in a genetic relation to one another, and the serial process is not rhythmically repeated in a genetic succession. Moreover developmental processes appear to have a reverse *direction* to those which characterize the radio-active stuffs. In the former there is increase in weight, in the latter decrease. One is a synthetic process, the other an analytical one. In one there is increase in complexity of some sort (i.e. 'elaboration') in the other

[1] It will be seen later that this is inadequate to state what is

there is decrease in complexity, either in the same or in a different sense.

The genetic relation is such that a given individual y which is genetically derived from x, was at one period of its history a *part* of x. In other words, as we trace back the history of y we find a period in its history which is also a spatio-temporal part of x. Or, we may put it in this way: Given an organism x, a temporal part of it (provided it does not 'die young') is such that it becomes divided into two spatially separated parts, one of which (in Metazoa) continues to persist as further temporal parts of x, whilst the other is regarded as the beginning of the history of y.[1] The successive changes which characterize the history of y are *approximately* a repetition of the changes which characterized x. Hence the genetic succession constituted by x, y, . . . etc., exhibits approximately a rhythmical character.

Now the necessity of introducing the qualification 'approximately' raises a problem. Why is it that the serial changes of y are not exactly a repetition of the successive changes of x? In the case of radium the changes which characterize one specimen appear to be an exact replica (as far as our knowledge goes) of those which characterize any other specimen.

1st Answer: Because the serial changes characterizing the history of any organism depend upon the environmental events which are contemporaneous with it. Therefore when we find that y does not repeat x exactly this is because the environmental events simultaneous with y were different from those which were simultaneous with x. In the case of radium the changes are indifferent to environmental conditions. But this will not do because y' as well as y may also be genetically derived from x, and y' may differ markedly from y in some definite respect, which cannot be correlated with environmental differences. Consequently this first answer must be amended.

2nd Answer: It is admitted that if y' and y have different environmental conditions they may be different, but since differences appear which are not correlated with environmental differences in this way, it follows[2] that *in such cases* they were

[1] After union with a part separated from another organism x', when sexual reproduction is involved.

[2] It 'follows' only if we admit the principle that differences cannot appear between two natural entities not previously different (apart from numerical difference) unless the contemporaneous environmental events of their histories are different, and that is a 'postulate', not a logical necessity. Cf. p. 220.

immanently different. In other words: y' and y differed *throughout* their histories, and not merely during the later durations when they were *perceptibly* different. Thus the organism during its history exhibits a series of changes of its characterization which is approximately similar to the series characterizing other individuals genetically related to it. But its characterization is dependent partly on environmental conditions, and partly on factors (whatever their nature may be) immanent in itself, and the factors immanent in two organisms closely related to one another may be different.

So far, then, we may say that development is the name given to the serial change in the mode of characterization of an organism in the course of its history, or to the serial changes exhibited by the mode of characterization of an event which is knowable as an enduring organism, which is an individual member of a succession of such individuals in genetic continuity, each individual of which exhibits approximately the same serial changes. And such serial changes are dependent, in a given individual, on (1) the contemporaneous environmental events, and (2) on factors immanent in that organism. But we have not so far said anything about what such factors may be or what precisely 'immanent in' means.

The doctrine of preformation, in its original form, appears to have identified the 'immanent factors' with the *whole* of the newly separated individual, and to have supposed that this simply underwent a process of increase in volume. In its modern form the doctrine appears to identify the immanent factors with spatial *parts* of the newly separated organism, which are manifested throughout its history, and to regard such parts as being related to the mode of characterization of the individual as *cause is related to effect*.

But the notion of immanent factors arises, as we have seen, when we compare one individual organism with another which is genetically related to it, but which exhibits differences in its mode of characterization which cannot be correlated with environmental differences. Thus the doctrine of immanent factors is primarily concerned with the differences between different organisms or between different races of organisms. And this constitutes the subject-matter of genetical study. But the study of development, on the other hand, is one which, theoretically at least, could be pursued without reference to other organisms, just as physiology is a study of the individual

organism, which could be pursued without reference to other organisms. The development of the individual is a serial process and it is this serial process which is the subject-matter of embryological study. The theory of development is, therefore, concerned, not with the differences between one organism, or one race of organisms, and another, but with the differences between one duration and another containing different temporal parts of the same organism. Thus genetics and embryology are two quite different studies which should, at the outset, be pursued without prejudice to one another. In other words: the difference between a man and a monkey is one topic of study and belongs to genetics, but the difference between a man when he is an egg and the same man when he is twenty-one years of age is quite another topic of study and belongs to embryology. We require, therefore, first to examine the serial process which constitutes development more closely to see what it involves, and then to examine the present state of opinion regarding the genetical question. We shall then be in a better position to understand their relations to one another, and also to understand the nature of the antithesis between preformation and epigenesis.

Modern biology—unlike eighteenth century biology—recognizes two kinds of development: (1) individual development and (2) racial development or evolution, and the antithesis between preformation and epigenesis is liable to arise in regard to both kinds of development. Consequently the discussions of this chapter fall naturally into two divisions—the first dealing with this antithesis as it concerns individual development, and the second dealing with the problem in relation to racial development.

Division I: Individual Development

Our first duty is to get a clear notion of what happens in individual development, and I shall therefore endeavour to give an empirical description of it on the basis of the discussions in the foregoing chapters, making use of the terminology elaborated in the sections dealing with substance, causation, and organization. This will naturally involve modes of expression which differ from those now current but I hope

this will be compensated by a gain in clarity and by the advantages resulting from viewing old problems in a new light. We shall have to try and find an answer to the questions: What exactly is it that develops—what persists, and what changes?

The individual living organism is an event—a spatio-temporal happening. For human experience it is *known* as a certain perceptual object, namely a given animal or plant of every day life. And this perceptual object is a character of an event. We *know* such an event as a persisting thing, that is to say we know the event which constitutes the organism as having a certain permanence of characterization of a certain type. It is of the type which requires time in which to display itself, since successive temporal parts of the event are differently characterized—it is a non-uniform object of the serial type. In other words the perceptible organism pervades a historical route,[1] or bundle of historical routes, and we are concerned with the earlier parts during which, as it is commonly expressed, the organism exhibits a progressive increase in complexity of form, and the term development is usually only applied to this period.

We will begin the description of the developmental period in animals with the fertilized ovum in the usual way. This is commonly referred to as 'a cell' but for reasons already given (p. 296) I should prefer to say that it is the character of the event which is known as the developing organism which characterizes its earliest temporal parts, those namely in which it exhibits the cell-type of organization. We must guard against confusing a whole with a part. If now we consider successive small slices of the history we find that this cell-type of organisation is not merely repeated in successive temporal parts, but is also spatially repeated, so that in later slices we have the event which is the organism characterized by a different type of organization, namely two contemporaneous cells often related to one another in a definite manner and to some plane of an earlier slice. Thus the cell-type of organiza-

[1] For definition of 'historical route' see Whitehead, *Principle of Relativity*, p. 30: 'A route lying entirely in one moment is called a *spatial* route, and a route which lies entirely in the past and future of each of its event-particles is called a *historical* route.' An event-particle is an event with all its dimensions ideally contracted, and a mat (for a given time-system) is a duration with its ti
so restricted.

tion now characterizes not the whole but each of two spatial parts of the event which is the organism. This spatial repetition, with temporal passage, of the cell-type of organization continues, and constitutes the type of change we observe during the period of cleavage. At the same time it must be remembered that, in some organisms at least, the distribution of the parts may be experimentally disturbed without impairing the 'normal course' of later periods. During the period of cleavage, then, the organism, from being non-cellular becomes cellular, i.e. its spatial parts are now cells, i.e. exhibit the cell-type of organization, whereas before they were not—they were cell-parts. Moreover we now not only have cells as parts but cells in certain perfectly definite relations to one another. Consequently it does not now suffice to speak of the organism considered as a perceptual object as simply an aggregate of cells, but always as having an organization *above* that of a cell, as having, that is to say, cells as characterizing *parts* in certain definite organizing relations.

The next step is the appearance of *cellular* parts, i.e. of distinguishable spatial parts which are themselves constituted of cells. This happens in the process of gastrulation. As this series of changes continues we can discriminate parts of a new order—germ-layers—and thus a third type of organization appears as characterizing the whole during this period. We have, namely, the gastrulation pattern as the type of organization of the whole, and in this we can now discern two parts —say ectoderm and endoderm—in a definite spatial relation to one another. And these are themselves organized, their parts being cells (i.e. 'known as' cells) in a definite relation to one another. Thus we have already a hierarchy of parts in organizing relations: (1) cellular parts (germ-layers); (2) non-cellular parts (cells); and (3) cell-parts (chromosomes, Golgi-bodies, etc.). From now onwards every part of the organism can be put into one of these categories. Moreover it should be noted that certain types of parts have been, and continue to be, present throughout, namely, cells and their cell-parts, i.e. the cell-type of organization. Now certain cell-parts *do not develop*, e.g. chromosomes, neither do they exist except as cell-parts. Thus the cell-type of organization appears as the irreducible minimum which forms the starting point, and is spatially repeated by division, to form the spatial pattern characterizing a given period, with its parts in a

definite relation to one another. With each division new parts and hence *new relations* come into being. But over and above division there is a progressive building up of parts of the cellular order, themselves in definite relations. Thus we can now say precisely what it is that constitutes development, and what it is that develops. Clearly we can say at once that what develops—what changes—is the *mode of organization* of the event which is the organism. The ovum does not develop into the embryo. The ovum is a character of a temporal part of the event which is now past. The event does not change except in the sense of receiving accessions of new temporal parts with the passage of time—the creative advance of nature, as Professor Whitehead calls it. But the new temporal parts exhibit a progressive change in their spatial characterization, indicative of the coming into being of more and more elaborate types of organization. And this is achieved by a spatial repetition of the cell-type of organization, and that in its turn is achieved in virtue of the peculiar property of this type of organization to divide into two spatial parts each having the same type of organization and the same intrinsic properties, or 'immanent factors'.

Now in one sense of the term the process, so far as we have described it, is an epigenetic one. At the end of, say, gastrulation, we have a type of organization which was in no sense 'preformed' at the period with which we started, since this type of organization involves cellular parts, and such parts did not exist during that period. On the other hand, what did exist then, and in that sense was 'preformed' was the cell-type of organization, which still exists now, but as characterizing not the whole but certain repeated parts. Thus we have all three notions—becoming, change, and persistence illustrated: the event *becomes* because it is an event; the type of organization of the whole *changes* in a certain direction, i.e. develops; and the cell-type of organization *persists* by spatial as well as temporal repetition. Also the organism as one substantial thing persists, i.e. there is continuity in the mode of characterization of the event which is known as the organism in spite of the change of its organization, and through this continuity the race persists.

We come finally to a fourth type of developmental process—the appearance of differences between cells apart from their differences in their relations to one another, i.e. the *elaboration*

of new cell-parts, or what is commonly known as histological differentiation. This is a process which may be encountered during early periods but is especially characteristic of the later ones. Thus if we consider the primitive gut it is at first a cellular part having perhaps from an early stage two types of cells in a definite relation to one another and constituting two cellular parts, namely an inner epithelial layer and an outer mesenchymal layer. Later the outer layer undergoes further elaboration into two different cellular layers in one of which the component cells are transformed into smooth muscle cells, and in the other of which blood-vessels and connective-tissue cells are elaborated. In the case of histological differentiation we again evidently have an epigenetic process since the typical organisation of a given tissue cell was not *pre-formed*. This is clearly shown by transplantation experiments. On the other hand, cell-differentiation of early stages in which, say, yolk-laden cells are differentiated from yolk-less ones is evidently a process of sundering of parts differently characterized, but not one of *elaboration*[1] of new cell-parts.

We now require to consider some of the major results which have been reached by the experimental study of developmental processes.[2] Such an experimental study usually takes the form of either:

(1) Seeing what happens when the mutual relations of the parts are disturbed, or
(2) Seeing what happens when the 'normal' environmental conditions are disturbed.

1. One result of the first method has been to show how important is the difference between part and whole. If a part is separated from an embryo it either behaves as a whole or it perishes (perhaps after exhibiting some further developmental changes). It follows the 'All or Nothing Law'. In some cases the two blastomeres of the two-celled stage can be separated and each behaves as a whole. This may also be

[1] This is one reason for preferring the term *elaboration* to *differentiation*.
[2] For a short readable account of these results the non-biological reader may be recommended to consult Mr. de Beer's *Experimental Embryology*, Oxford, 1926.

true of blastomeres of later cleavage stages. Conversely two wholes may be made to fuse to form a single whole. In all cases of separation what is separated must be at least at the cell-level of organization if it is to persist. A part behaves as a part so long as it is in an organic relation to other parts. Destroy that relation and it either behaves as a whole or it perishes. In one case it has an organic environment in another an inorganic one. If the part separated has an organization above the cell-type it will exhibit restitutive changes if it persists.

A second result has been the discovery of the distinction between what we may call 'indifferent' and 'self-differentiating' parts. Indifferent parts can be interchanged and the changes exhibited by them vary according to their relations to other parts. In 'self-differentiating' parts, on the other hand, the serial changes they exhibit do not appear to differ when they are transplanted to abnormal situations. Their 'fate' is said to be 'herkunftsgemäss',[1] whereas that of the former kind of part is 'ortsgemäss'.[2] This applies also to regenerating tissues in some cases. In the experiments of Dr. P. Weiss a young piece of indifferent regenerating tissue from the stump of the amputated tail in the newt developed into a leg in some cases when it was transplanted to a 'leggy' situation. If, however, the transplantation was carried out later, when the development of the regenerating tissue had progressed further, it developed into a tail in the abnormal situation. Thus we may say that in those organisms in which these facts have been worked out there are periods in the history which contain spatial parts whose characterization depends on their relations to other parts,[3] but that as successive temporal parts are realized new spatial parts are elaborated whose further history in respect to developmental changes is not variable in accordance with their relations to other parts. Moreover a piece of tissue from one embryo of a given species can be transplanted to a different region on a specimen of another species and although it exhibits its specific characterization throughout it will take its place in the new situation

[1] i.e. according to their place of origin

[2] i.e. according to the place to which they are transplanted.

[3] But not in respect to characters depending upon the intrinsic or 'immanent' properties of the cells of the race of organisms to which it belongs. See below, p. 383.

and behave 'ortogenetics', just as in the case of transplantation to an embryo of the same species.

2. Experiments involving departures from the normal environment have shown that various chemical ingredients are essential for normal development. Certain marine eggs for example require calcium if the blastomeres are to remain in their normal biological relations. Excess of magnesium chloride results in cyclopian monsters in certain fishes, and the same result may be observed under other abnormal environmental conditions. Speaking generally many deviations from the typical environmental circumstances result either in the destruction of the organism or in quantitative variations in the developmental processes, e.g. acceleration or retardation of cell-division according to temperature. The environmental events exhibit certain broad unchanging or rhythmically changing pervasive features, but over and above this there is a good deal of change which is contingent. Consequently no two organisms can be said to have exactly the same environmental conditions, although in organisms which develop in a shell or *in utero* a closer approximation to similarity is presumably reached. There appears to be a certain range of variation of environmental conditions within which developmental processes do not exhibit deviations, but it is difficult to say how close the relation is on account of the difficulty of estimating the 'sameness' of two organisms.

Although *embryology* is primarily concerned with the processes exhibited by the single individual organism as such it presupposes a race for obvious reasons. It works with the concept of 'normal' development, and this can only be reached if there is available a supply of embryos all manifesting much the same series of changes. Moreover a given embryo manifests its series of changes once only and consequently an experiment can only be repeated on another specimen which is presumed to differ only numerically from the first 'for all practical purposes'. *Now genetics*, on the other hand, does not appear to be at all concerned with developmental *processes*, and would be quite impossible if only one embryo were available for study, because it is concerned with the *differences* between *organisms* which are genetically related to one another.

It studies the different *characters* of related organisms and it studies the origin of the differences between their characters. Suppose an organism O'' has a part P characterized by b, it is impossible from mere inspection to say anything about the nature of such an organism from the genetic point of view. Its genetic properties can only be discovered by examining O'' in its place in the genetic series O', O'', O''' —if for a moment we omit complications involved in sexual reproduction. Suppose it is then found that all members have Pb we should have a race which was unvarying in respect to that character. If, however, it was found that the predecessors of O'' all had P characterized by w whilst its successors all continued to have Pb we should speak of mutation. But this would only be true on the assumption that the environment E has been constant for all the members. If we have $EO'Pw$ and $E'O''Pb$ the difference between O'' and O' would be regarded as a consequence of the difference between E' and E and we should speak of modification or 'continuous variation'.[1] But not only is the environment, the spatial part and the character required to be specified, but also the particular duration of the history known as O'' during which the character is manifested, is involved. Geneticists have chiefly dealt with adult characters but characters manifested during other periods have been studied and from the theoretical point of view it is obviously quite arbitrary to limit genetical study to the adult periods. Consequently the full formula for comparison of one history with another is $EOPcD$ where c stands[2] for the character and P and D for the spatial and temporal parts characterized, and E for the contemporaneous environmental conditions, which may or may not have been uniform. We appear to have two variables E and O upon which the character c of PD depends. A variation in either may result in a change in c. The problem is how we are to conceive O in order to interpret the results observed by geneticists. O is an event of which the PDs constitute part-events in organic relation. Similarly E is a

[1] This assumes, in accordance with the causal postulate, that the relation between organism and environment is simpler than appears to be the case. Cf. pp. 220 and 332.

[2] More correctly c stands for a determinate character under a given determinable, e.g. red under colour, 1ft. under length. See the useful terminology of Mr. W. E. Johnson in his *Logic*, Part I, p. 175; Part III, p. 84. A pair of 'allelomorphs' is a pair of alternative determinate characters under the same determinable.

complex of events the part-events of which are not related in organic unity. It is also clear that the character *c* stands in a relation which is at least four-termed involving both *PD* and the relevant part-events of *E* as well as other *P*s.

Let us compare the above with a case from chemistry. Consider a crystal of copper sulphate. If it is in a dry stoppered bottle we speak of it as 'existing in a state' and exhibiting its qualities—colour, shape, etc. If it is dissolving in a vessel of water it is manifesting a property. We speak of it as a particular specimen of a chemical substance. If we are given another specimen and are in any doubt about it we place it in various situations and observe its changes. If under the same conditions it exhibits the same characters as the first specimen we pronounce them both specimens of the same substance. If they behave differently we either regard them as different substances, or we look more closely into the conditions to see whether these have varied. This procedure is exactly paralleled by the procedure of the geneticist. In all cases the characterization observed is a manifestation of the 'properties' of the thing *and* of its environment.[1] The dissolution of the crystal in water is a property of *both* crystal and water. Similarly the characters of the organism are really characters of the organism *and* its environment.

Moreover, just as the crystal is an exemplification of a chemical substance in the 'secondary meaning' of substance in Aristotle, so an organism is an exemplification of a secondary substance—a 'biological substance'—namely the race to which it belongs. And just as the chemist distinguishes pure and compound substance, so also the geneticist recognizes pure and mixed races. The comparison can be carried further still. A specimen of an elementary chemical substance cannot be altered (except in the case of radio-active elements which change spontaneously). And similarly, although the geneticist can change mixed races by crossing, the pure races appear to be extremely stable and the changes called mutative changes are rare. Thus the problem is to know how we are to conceive this extremely stable entity whose manifestations we see in the observable characters of individual

[1] But not necessarily of the simultaneous environment for a given brief duration. The past environmental conditions may be correlated with an enduring change in the characterization of an organism.

organisms which are, of course, manifestations of both organic and inorganic environmental events.

We must now recall the important differences between the chemical and the biological 'secondary substances'. The individual specimen of a biological race is genetically related to other specimens, and to one or two in particular it is related as 'immediate successor'. This is not the case with crystals. Moreover a given crystal, e.g. quartz, in nature merely 'persists in a state' in its normal environment, or such changes as it may exhibit are directly correlated with contingent environmental changes. A living organism on the other hand manifests serial change in a constant normal environment. These are the two principal differences which are responsible for the fact that there is neither embryology nor genetics in chemistry.

It is evident that in both cases—both chemical and biological 'substances', we think of something persisting in spite of the changes of the characters, and this is regarded as being in part responsible for the persistent regularity of those changes. It appears that a given event which is knowable in this way has some persistent property upon which its characterization depends and perhaps this is what Professor Whitehead means when, in connexion with perceptual objects, he speaks of the 'control of ingression' of sensa into events. Now an organism, as we have seen, exhibits a developmental series of changes, in which its different spatial parts increase in number and in their mode of organization, so that greater and greater diversity of characterization becomes possible, and the question we shall have to consider is the question whether that which is responsible for the characterization is also that which is responsible for the regular series of changes which constitutes the increase in organization. But meanwhile there is another point of comparison with the chemical substance to be considered. Both are regarded as particulate or atomic. But in an organism only a part of the minimum type of organization which is capable of independent existence is regarded as being the seat of that upon which the persistent properties depend, namely the chromosomes, since it appears to be this which is strictly continuous in the genetic series, and, in sexual reproduction, is contributed equally by both sexes. In both chemical and biological examples differences in characters in a constant environment are interpreted as

resulting from differences in particles either in kind or arrangement. But the whole crystal is regarded as *composed* of these particles, whereas in the case of the organism only *part* is supposed to be composed of the particles. Mendel, of course, did not go so far as this. All he supposed was that if an organism has differently characterized off-spring the germ-cells which were their earliest temporal parts were different. But since the germ-cells in a given individual are required to be different in order to account for the distribution of characters in the offspring, and since there is every reason to believe that the divisions of cells ordinarily are equal (i.e. purely quantitative) it is believed that the peculiar division which precedes the formation of the ripe germ-cells is a qualitative one in which differences between germ-cells are established. There will be as many possible different germ-cells as there are different possible modes of division. And in order to allow for these different modes of division the chromosomes are regarded as particulate, and in cases where the expected numbers are not observed it is necessary to assume abnormalities in the usual mode of division. Where sexual reproduction occurs each individual specimen of a biological 'substance' will in fact be double and it will be pure if the set contributed by each parent is the same. But that one set is sufficient is shown by cases of the occurrence of parthenogenesis in which only one is involved.

It would be extremely difficult at present to form any opinion regarding the precise meaning to be attached to the assertion that the 'immanent endowment', upon which the persistent properties depend, is particulate or atomic. This would have to be discussed as a special instance of the wider question of the meaning of the atomic character of events. We obviously must not confuse the visual appearance of the chromosomes which is itself a character with what is thereby characterized. There is one more point of contrast with chemical science which is worthy of note. If you have a specimen of an elementary substance, say iron, a given volume is always associated with a persistent measurable character e.g. wright, even when it enters into compound substances, and it is very largely on this fact that chemistry depends. But in biology it has not so far been possible to discover any such direct and simple relation between a measurable character and a given volume of the 'biological substance'.

We have now to essay the extremely difficult task of investigating what is involved when we try to bring the results of genetics into relation with the results of embryology. One important point should, I think, be constantly borne in mind. Embryology deals with the process of development in a given organism on the assumption that this is typical of its race. It deals with the differences between successive temporal parts or slices of the *same* organism in so far as these exhibit a progressive increase of organization. Genetics, on the other hand, deals very largely with relatively minute differences in the mode of characterization of a given spatio-temporal part of one organism as compared with comparable parts in other organisms belonging to the same race. Hence the factors immanent in the history of each organism are primarily concerned with the mode of characterization. It would appear, therefore, that the question of the relation between genetics and embryology is a question of the relation of organization to characterization. Organization arises out of the relations between parts. Characterization is expressive of differences between the parts in their organizing relations.

Owing to the importance attached to the *nucleus* by the geneticists it is not surprising that it has also attracted the attention of embryologists, and that attempts should frequently have been made by them to discover in it a key to the understanding of differentiation in development. But at the present day these attempts appear to have failed, and it would not be a misrepresentation of the general opinion to say that the view most commonly held now is that nuclear divisions are usually equal except at *meiosis*. In other words it is believed that apart from the *mature* germ-cells the nuclei throughout the body are equivalent in their chromosome constitution. Professor Wilson says:

> "even in a highly differentiated type of cleavage therefore, the nuclei of the segmenting egg are not specifically different but contain the same materials in cells that undergo the most diverse subsequent fate."[1]

And speaking of Driesch's pressure experiments on page 1059 he adds:

[1] *The Cell*, p. 1061.

> 'specification of the blastomeres cannot, therefore, be due to specific nuclear differences produced by a fixed order of qualitative nuclear divisions but must be sought in conditions of the cytoplasm.'

This being the case attention was turned to the cytoplasm, and great hopes were attached to the discovery of visible differences in different regions of the cytoplasm in certain eggs. But this attempt also has not met with the success which was hoped from it. It was found that in many cases such stuffs could be centrifuged away without disturbing development. The 'visible materials' are not the 'true organ-forming substances', says Professor Wilson, but:

> 'secondary products of the cytoplasmic activity which can at most be regarded as only external signs or indices of a more deeply-lying organisation of the clear ground-substance or hyaloplasm of the egg.'[1]

Thus the notion of organ-forming substances is not given up but refuge is taken in invisible 'structureless fundamental ground substance'. But there are great difficulties in this hypothesis also. Professor Wilson himself points out on p. 1079 that:

> 'the germ-regions can be thought of neither as sharply delimited nor as absolutely fixed areas. The egg is not a fixed mechanical structure; it is a plastic protoplasmic system comprising substances that tend towards a certain grouping in the egg. The predetermination of the germ-regions is primarily qualitative; though a quantitative factor is also present it is in considerable degree subject to regulatory control.'

When later Professor Wilson comes to the organ-forming substances and the centrifuge experiments he says:

> 'The difficulty of conceiving how the preformed organisation of the egg can be bound up in a liquid or semi-liquid substance, such as the hyaloplasm often seems to be, is obvious. Lillie and Conklin have accordingly argued in favour of a relatively firm condition of aggregation in the hyaloplasm yet one of such a nature that the cytoplasmic inclusions can still move through it.'[2]

This looks very much like having the best of both worlds. It would seem that the desire for stuff were being pushed too far, leading to contradictory postulates. Even if 'organ-forming substances' are admitted it does not appear that they help us very much. At most they would seem to indicate that in some eggs certain blastomeres instead of being equivalent are differently characterized in such a way that each is

[1] *The Cell*, p. 1067. [2] *The Cell*, p. 1094.

essential for normal development. And in spite of all that has been said about the cytoplasm Professor Wilson, at the end of his book (p. 1116) says: 'For all we know localization may be determined by the nucleus, or at least by a process in which the nucleus is concerned.' But if, as seems very probable, the nucleus divides equally and is concerned in every vital process this does not help us to decide anything about the developmental processes in particular.

When nucleus and cytoplasm fail it is the custom to turn to the environment of the egg but this again has proved of little avail. In birds and mammals the environment is probably remarkably uniform and can therefore hardly be invoked to account for the progressive increase in organization during development. Professor Wilson's statements on this point seem to be very contradictory. On p. 1036 he says:

> 'We shall treat the external factors as conditions of development rather than primary or determining causes.'

But at the end of the book, just after the passage quoted above regarding 'localization' being 'determined by the nucleus' Professor Wilson says:

> 'There is good reason to hope for further light on the problem from the study of the relation of the developing germ to external conditions.'[1]

And yet, only six pages before this he has also said that 'external formative stimuli are but limiting conditions to which the egg adjusts itself'. But for reasons already given it does not seem that an appeal to the environment will help very much. Thus the whole question is in great confusion and, as Professor Wilson himself says: 'It must be admitted, therefore, that the mechanics of localization still present an unsolved problem.'

These difficulties strongly suggest that something important has been overlooked. The embryologist seems to be caught in a circle of nucleus, cytoplasm, environment and back again to nucleus, from which he does not escape. We must therefore try to discover the root of the difficulty as a preliminary to its removal. In the first place it is clear that rather crude notions

[1] *Op. cit.*, p. 1117.

about causation are being used uncritically by embryologists. Professor Wilson, for example, in one of the above passages, seems to distinguish between 'conditions of development' and 'primary or determining causes', but he does not discuss the distinction, and its precise significance and justification seem doubtful. A distinction is also made by embryologists between a 'mere description' and a 'causal explanation' of development. There is a good deal to be done by way of analysing these distinctions carefully and making clear what they mean. Secondly we observe the usual failure to take organization and all that it implies sufficiently seriously. No attention is paid to the fundamental difference between whole and part. There is too great a tendency to think in terms of the doings of individual cells to the neglect of their mutual relations and in particular the possibility of internal relations is not considered. Thirdly space and time are not taken seriously, and finally the epigenetic character of the developmental process has been reluctantly if at all acknowledged. A step forward in the direction of overcoming these defects is made if organisms are interpreted in terms of events as organized to constitute with their characters the differentiated parts of a complex which is knowable as one enduring thing. Stated in terms of increasing elaborateness of organization the epigenetic character of the developmental process seems clear. Thus when we contrast the brilliant skill and ingenuity displayed by embryologists in investigating the developmental process, with the confusion in which the subject stands from the theoretical standpoint it is difficult not to conclude that what is lacking is a mode of thinking adequate to this region of biological fact. Embryology seems to be pre-eminently one of those sciences in which, in the words of J. S. Mill:

> 'if not only so little is proved, but disputation has not terminated even about the little which seemed to be so; the reason perhaps is, that man's logical notions have not yet acquired the degree of extension or of accuracy, requisite for the estimation of the evidence proper to those particular departments of knowledge.'[1]

This is not the place in which to attempt the construction of a theory of development,[2] but it will not be amiss to make

[1] *Logic*, Introduction, 6.

[2] For a good critical discussion of modern theories of development see L. Bertalanffy, *Kritische Theorie der Formbildung*, Berlin, 1928.

some suggestions towards a removal of the above difficulties, assuming the diagnosis is correct. First, regarding the epigenetic character of development: this has been obscured in part by the traditions persisting from the original cell-theory. My way of expressing the facts of development in terms of the cell-type of organization as being spatially repeated in division brings out clearly this epigenetic character, because we see at once that the very first act of cleavage raises the level of organization. For after this act we now have the *whole* with two *parts both* exhibiting the cell-type of organization, whereas before we only had the *whole* so characterized.

Now this raising of the level of organization even one step involves and carries with it a great deal more. With the appearance of two new *parts* we have at the same time the appearance of new types of *relations*. This is clearly shown by many ova. In many cases there is every reason to believe that these two parts are quite equivalent in their intrinsic biological properties. Separate them and each behaves as a whole. Leave them in their normal relations and the parts resulting from their further division yield *different parts* of the one whole. But if this is the case we have no recourse but to interpret the differences between the two cases as resulting from the differences in the relations of the two blastomeres in the two cases. So long as they constitute parts of one organic whole so long are they different, although and in spite of the fact that one is not *intrinsically* different from another. Consequently the relation expressed as "being parts of one whole" is an internal organizing relation. Now this relation between two or more blastomeres is, whatever else it may be, a *spatial* relation. It has often been pointed out that the "fate" of an embryonic part is often dependent on its spatial relations. But embryologists have almost invariably disregarded such relations as being in any way responsible for differentiation. If, however, we regard spatial relations as *internal* relations, so that parts can be different *in virtue of* differences in their spatial relations this assumption might help us to overcome some of the difficulties of embryological interpretation. The traditional attitude of embryologists is well illustrated by the following passage from J. W. Jenkinson:

> "... the destiny of a part is imagined to be determined by its distance from the system of points already established, "its fate", so runs the famous formula, "is a function of its position in the

> whole". It would, however, be absurd to suppose that the behaviour of any one of a number of precisely similar bodies could depend upon its mere geometrical position. The points already differentiated—the animal pole, for instance—must be supposed to exert an influence with a force which is some function of the distance upon the parts which are at present equivalent, and so excite their differentiation."[1]

Now this is a very a priori way of proceeding. We cannot settle the claims of rival theories in natural science by calling one of them absurd. We have to find out what is the case. Why *must* the animal pole be *supposed* to 'exert an influence with a force'? As soon as we begin to talk about influences and forces we expose ourselves to the dangers of animistic metaphysics, whether we conceive them materialistically or vitalistically. At one time astronomers believed that the sun exerted an 'influence' on the earth but now, apparently, this way of expressing the facts is no longer considered 'necessary'. The same a priori way of proceeding is illustrated by the following from Bateson, who is usually more cautious:

> 'in order that two differentiated halves may be produced, some event must take place by which a chemical distinction between the two halves is effected."[2]

Here again there is no justification for the 'must'. One might as well say that the difference between a magnetized and a non-magnetized bar of iron *must* be a chemical one. There is little occasion for the use of the word *must* in natural science.

The traditional view regarding space has been that it is a kind of box with things in it and is such that the position of the things in the box makes no difference to them. Accordingly there was no need to take spatial relations seriously. But *if* it were the case that in such an entity as a developing organism spatial relations were internal relations then the position of a part in such a whole would be 'responsible' for the doings of that part in the whole. Professor A. N. Whitehead says:

> 'It has been usual, indeed, universal, to hold that spatio-temporal relationships are external. This doctrine is what is here denied.'[3]

Now it does seem that there is plenty of evidence from experimental embryology in favour of this view, and if the matter

[1] *Experimental Embryology*, Oxford, 1909, p. 282.
[2] *Problems of Genetics*, p. 33.
[3] *Science and the Modern World*, p. 133. Chapter VII of this book is extraordinarily full of suggestions for the embryologist.

were approached experimentally with such a view in mind more evidence might be forthcoming. Dr. Weiss's experiments on transplanting young regenerating tissue from the tail to the limb region strongly suggest the importance of spatial relations in the whole. With regard to temporal relations I am not aware of any experiments bearing very clearly on this point. Two isolated blastomeres from the two-cell stage both belong to the same temporal part of the organism from which they are taken. We know that a part may in some cases be transplanted to another organism and will develop according to its new spatial relations. But suppose it could be transplanted to a new organism so that its temporal relations to the whole were different, then if other factors were not involved, we should expect its fate, if temporal relations are internal, to be altered in accordance with its new temporal relations. In such an organism as a newt, in which a high regenerative ability is retained throughout life, what is regenerated depends not only upon the spatial relations of the regenerating tissue but also on the temporal slice of its history which has been realized. in other words what is regenerated depends on the state of development to which the organism has reached. If the animal already possesses legs the regenerate grows into a leg. Now if such a germinating blastema were transplanted to a younger newt without legs, and if the fate of such a blastema is a function of the temporal relations in which it stands to the whole we should expect it to behave in accordance with its new surroundings not only spatially but temporally. Moreover spatial relations are susceptible of exact repetition on different scales, and this may help us to understand how the same spatial relations can be effective in a rounded off part of the wall of a blastula and thus aid in the interpreting of 'harmonious equipotential systems'.

On the other hand it would be a great mistake to fall into the common error of expecting one factor or set of factors to explain everything, and there is no occasion to decry the importance of quantitative differences between blastomeres. These appear to be important in such processes as gastrulation, for example in *Amphioxus*. Moreover the shape of blastomeres in their normal relation is often different from what it is when they are separated, and with this the plane of cleavage is connected, and on the planes of cleavage depend the spatial relations of the parts resulting from this process. Two very

important questions are clearly (1) how is the onset of cleavage divisions dependent upon internal and external events, and (2) how is the highly specific cleavage pattern dependent on external and internal events. These questions have of course been studied in detail but so far no satisfactory answer to them has been reached.

As development proceeds a new type of part is elaborated and a further step up in the level of organization of the whole is achieved. That is to say we have so far only had parts exhibiting the cell-type of organization. But we soon begin to get 'transcendence of cells'[1] so that groups of cells come to constitute parts of a higher order, namely germ-layers or *cellular* parts. And with the attainment of such parts we now have the possibilities of new types of relations between one *cellular* part and another. Moreover within these cellular parts further elaborations occur in accordance with their relative spatial positions. And when such parts are established we have asymmetrical relations between one cellular part and another, by which I mean that the part A may be dependent upon B, without B being dependent upon A. We have examples in Professor Spemann's organizator and in the familiar example of optic cup and lens in certain amphibians. It is evident that spatial relations are of greatest importance during early stages when so-called 'plasticity' is at a maximum.

In the period of 'rigidity' which follows it would be a mistake to suppose that the parts are unrelated and independent upon one another. They are still in organic relations as parts of one organism, and all that the term 'self-differentiation' expresses is the fact that in respect to their morphogenetic changes these relations are not important. And within a given part the further elaboration of its parts may well involve such relations in the same way as was the case with the larger parts in an earlier period. Finally when we come to intracellular elaboration it is important to remember that this is not simply an affair of what is going on in single individual cells or merely in aggregates of cells but involves definite territories of cells in their relations to neighbouring territories and to the whole.

[1] See p. 308 above.

We come now to the question of the relation of the Mendelian factors, invoked by geneticists to account for the differences between one organism and another, to this process of development, in which the problem is to explain the gradual increase in organization, i.e. the difference between one slice of a history and another. Now genetic factors are primarily concerned with characters, and the organism is characterized throughout its history. Characters are not something stuck on to an 'organism in general' at the end of the history. If Mendelian factors were simply concerned with the characters at a given slice of the history and not with the organization it might be easier to understand their relation to the developmental process, for we should then be able to say that such factors had nothing to do with the mode of organization and its gradual elaboration in development. The fact that an embryonic part transplanted to another species may behave "ortsgemäss' in its new situation, and yet exhibit its specific characters, shows clearly that its mode of characterization in one sense is immanent in it, but that the particular rôle in the developmental process is dependent upon its spatial relations to other parts and is not immanent in it. The characterization of the *whole* is an expression of the immanent endowment of the race to which it belongs (for a given environment) but (in early stages) the particular contribution of a *part* depends on its relation to the whole, and if that part is in relation to a whole of another species it will still exhibit the same plasticity but without departing from its own 'species specificity'. The separated part in such cases does not behave as a whole, because its strange host provides an organic environment for it in relation to which it can still persist and exhibit its own specificity, although as a part of the new organism, and perhaps as a part which it would not have formed on its original site. It seems that every cell, in virtue of its being part of an organic race and having the cell-type of organization has a certain immanent endowment. If it 'develops into' a whole that whole throughout its history will exhibit its appropriate immanent characterization (for a given environment). But if it remains a part, its cell-descendants will behave as parts, and will be subject to the 'plasticity' exhibited by parts at a

certain stage. When that stage is past and its 'rôle' is fixed then it will exhibit the characterization immanent in it (for a given environment).

It is considerations of this kind which suggest that the immanent Mendelian factors have to do with one aspect of the process of development, namely characterization, and not with the other, namely organization. Does development i.e. increase in organization, mean production (by division etc.) of more and more parts in a hierarchy of organizing relations, and are the genetic factors only concerned with the mode of characterization of such parts? Are there two fundamentally different kinds of character: one depending upon the mutual relations of parts, and another depending on immanent (Mendelian) factors, and both of course depending on environmental factors? We now have to consider whether it is possible to maintain such a distinction.

The first requisite is obviously to determine what we are to mean by a 'character'. Bateson, with his usual insight very rightly remarks:

> would constitute a great advance in biological theory."[1]

Let us take a glance at some instances of geneticist's characters. Prof. H. H. Newman speaks of a character as 'one of many structural or functional details that characterize an individual or a race'. Note the word 'details'. Are we to understand by this that the colour or length of a man's nose is a character but not the nose itself? Geneticists do not as a rule trouble about noses but about why noses are short or long, blue or red, and so on. But this may be merely because you can cross two animals which only differ in the length of their noses but not two which differ in respect to the possession of a nose. Professor Przibram distinguishes three kinds of characters: morphological, (shape, colour, etc.), chemical (protein composition, etc.), and physiological (temperature, osmotic pressure, mode of locomotion, etc.).

[1] *Mendel's Principles*, Cambridge, 1909, p. 273.

But this seems to be a classification drawn up with regard to practical convenience rather than theoretical importance. If we examine a list of characters which are found to 'mendelize' we find them very varied: tallness in plants, branching habit, hairiness or glabrousness, shape of pods in peas, shape of leaves, biennial or annual habit, susceptibility to rust-disease, relative length of parts, shape of seed, various human defects, e.g. brachydactyly, hairlessness in mice, 'waltzing' in mice, length of hair, shape of comb (in fowls), colour of parts—eye, hair, petals, scale of insects, banding of Helix, 'sex', etc. The items in this list fall into three groups: (1) sensible qualities, e.g. colour, shape, etc.; (2) parts, e.g. hairs, meristic characters, etc.; and (3) 'properties'. Now if only the first and third groups were included under 'characters' it would be possible to maintain the distinction suggested at the end of the last section. We could then say that the Mendelian factors were not concerned with parts but only with the characterization of parts. But if *parts* are to be included then this distinction could not be maintained, for parts are spatio-temporal entities. The question, therefore, whether parts are to be treated as characters has important theoretical consequences.

Now in the above list the criterion which decides the inclusion in it of each of the items is whether they 'mendelize' or not. This is taken to imply dependence on genetic factors. But we might proceed in another way to determine what we are to call characters. Genetic factors are invoked in the first instance to account for what is persistent, pervasive or recurrent in the race. Accordingly whatever is persistent or recurrent will be a character. Let us consider a particular case. A frog-history during its earlier strands has no part that can be called a leg. But gradually it begins to exhibit a part which comes more and more to resemble what is known as a leg. The same thing happens in its ancestors and descendants. Are we then to say that legs are persistent features of the frog-race and are therefore characters? I think not. What persists in the race is not the part which is knowable as a leg since this comes and goes. What is recurrent is the 'legginess' of parts in virtue of which they are knowable as legs. 'Frogginess' also persists, but it requires time in which to display itself, and legginess is a 'part' of frogginess (in the logical sense) which

characterizes only certain stretches of a particular frog-history. Moreover, 'legginess' characterizes many other organisms, accordingly it is the 'frog-legginess' which is characteristic of frog-histories. It seems therefore that it is possible to preserve the distinction already pointed out between development as a process of gradual realization of characterized spatio-temporal parts, and the Mendelian factors as concerned not with this but with the characterization of the parts. Such factors will be concerned therefore with the 'frog-legginess' of frog's legs but not with the production of the part so characterized. But the frog all through its history is characterized and consequently whatever characters it may have will be dependent upon the immanent Mendelian factors. We again encounter here the peculiar difficulties attaching to the difference between part and whole. The whole history can and should be called a 'frog' because it is purely arbitrary to confine that term to the adult strands, but when can we speak of a leg? There are early strands when no one would say that there was a leg and there are later strands when no one would hesitate to say that the frog had legs. But in between these extremes is a vague period when we have to speak of leg-buds, leg-rudiments, etc. Thus although the whole is always a frog, a given part is not always a leg. The parts gradually come into being but the whole persists through a gradual increase in organization, given a favourable environment.

Now length of a part is a Mendelian character of that part and if we imagine such a character progressively diminished we should reach a condition in which the part was not recognizable. Such a condition could be treated as a case of 'absence' of the part and would mendelize. In this way we could treat such cases as hairiness and glabrousness in plants without departing from the fundamental difference between part and character. We should still be dealing with length which is a character or, better, a 'determinable.'

In this place it will be desirable to refer to a peculiar notion which has arisen in regard to characters. The fact that Mendelian analysis has for obvious reasons dealt very largely with adult characters has led to the extraordinary belief that the nucleus is concerned with 'species characters' and the cytoplasm with characters of the order, class, etc. Mr. de Beer for example says:

'The nucleus is the bearer of factors which influence, among other things, the more minute characters of the organism; the broader features of the embryo are controlled by factors in the cytoplasm of the egg.'[1]

Prof. Wilson refers to the 'singular notion' of Loeb that the egg is 'the embryo in the rough' and adds that such statements are 'rhetorically effective, but will not stand the test of critical analysis'. McClung also says: 'It is illogical and against the evidence of development in organisms to separate their structural features into categories of racial and specific characters.'[2] If such a supposition were correct it would lead to rather curious consequences when brought into relation to the theory of evolution. Consider for example the hair and mammary glands which are class characters. According to present accounts these will have appeared as mutations and will then presumably have been species characters depending upon 'factors' in the chromosomes. But in the course of evolution they will have become class characters. How then did the factors get switched over from nucleus to cytoplasm? If the constancy of species characters is the sole concern of the chromosomes, which are accurately divided, how is the constancy—the still greater constancy—of the class characters guaranteed? Red characterizes the blood of an enormous number of animals. Is this dependent in all cases on 'factors' in the egg cytoplasm?[3]

It will be desirable now to examine some current opinions regarding chromosomes and their relation to characters. And it is desirable to bear in mind that chromosomes are evidently very important parts of the cell-type of organization quite apart from any special significance they may have in development and genetics. Prof. E. B. Wilson seems to speak with considerable hesitation on this topic. On p. 916 of *The Cell* he says:

[1] *Experimental Embryology*, p. 130.
[2] Cowdry, *General Cytology*, p. 665.
[3] Moreover would not such factors have to be present in every part of the cytoplasm since nucleated fragments of eggs can in some cases develop normally?

> 'No one familiar with these subjects now conceives the chromosomes as exclusive agents of heredity. . . . Chromosomes are to be regarded as *differential factors of heredity*, rather than as central governing elements; nevertheless it is often convenient to speak of them as "determiners", taking the rest for granted.'

Later, on p. 975, he writes:

> 'In what sense can the chromosomes be considered as agents of determination? By many writers they have been treated as the actual and even as the exclusive "bearers of heredity"; numerous citations from the literature of the subject might be offered to show how often they have been treated as central governing factors of heredity and development, to which all else is subsidiary. . . . Many writers, while avoiding this particular usage, have referred to the chromosomes, or their components as "determiners" of corresponding characters; but this term too, is becoming obsolete save as a convenient descriptive device. . . . It is possible that all the chromosomes, or even all of the units which they contain, may be concerned in the production of every character. . . . The value of the chromosome theory of heredity does not lie in our identification of this or that "determiner" or "bearer of heredity" but in its practical importance as a means of experimental analysis. In this respect, in the writer's opinion, the theory has the same kind of value as the molecular and atomic constructions of physico-chemical science; and the "mystical" and "unscientific" character ascribed to it by some writers is purely imaginary.'

Thus Prof. Wilson is inclined to take a cautious methodological attitude towards these questions and protests against such expressions as 'central governing agents' or 'bearers of heredity', etc. Professor T. H. Morgan speaks of characters as the products or effects of the immanent Mendelian factors and identifies them quite literally with parts of chromosomes. He says:

> 'We know how the factors carried by the parents are sorted out to the germ-cells. The explanation does not pretend to state how factors arise or how they influence the development of the embryo. But these have never been an integral part of the doctrine of heredity. The problems which they present must be worked out in their own field.'[3]

That is to say the immanent factors have originally been postulated to account for the distribution of characters in a genetic succession of organisms or generations, and therefore tell us nothing about the process of development. In another place Prof. Morgan says:

> 'It can not too insistently be urged that when we say a character is the product of a particular factor we mean no more than that it is the most conspicuous effect of the factor.'[4]

[3] *Critique of the Theory of Evolution*, p. 144
[4] *Ibid.*, p. 117; the original is all in italics.

Professor McClung is much more definite in his assertions. He begins by saying :

> ' The chromosome theory of heredity is a specific development of the more general one which postulates the existence within the organism of a special substance, the function of which is to transmit and control the development of characters peculiar to the group.'[1]

The general theory here referred to is that of Nägeli who conceived a ' substance ' called ' idioplasm ' which performed the duties mentioned. This is an example of the use of the notion of stuff borrowed from chemistry. The persistent rhythm of the race is conceived as a persisting material stuff. Prof. McClung identifies this stuff with the stainable perceptible parts of the nucleus or ' chromatin '. But the same objections apply to this notion of chromatin as were urged against ' protoplasm ' because, if the chromosome hypothesis is true, each chromosome will be an entity with a highly elaborate organization. Prof. McClung is much more definite than Prof. Wilson, for example he says (p. 666) :

> ' The question is no longer " Do the chromosomes act as character determiners ? " but rather " How do the chromosomes act as character determiners ? "'

Also on the same page he uses expressions which are condemned by Prof. Wilson :

> ' The chromosomes in some way direct and govern the rate and character of the cell's operations ; the result depends upon the nature of the chromosomes, the materials with which they react, and the physical conditions of the reactions. A difference in any one of these means a changed product.'

Similarly on p. 678 he writes :

> ' To assert that the chromosomes exert a guiding or directing action does not imply that they accomplish the whole effect, but is merely an effort to assign them their place in the series of reactions involved.'

It seems clear from these passages that what goes on in a cell is conceived on the same lines as what goes on in a test tube. You have so many ingredients and you get a certain result, change one of them and you get a different one. ' A difference in any one of these means a changed product '. But if this is so what becomes of the assertion that the chromosomes exert ' a guiding ' influence or ' directing action ' ?

[1] *General Cytology*, p. 611.

Are such expressions used by chemists in regard to the contents of test tubes? Moreover, and more important, if the above view is correct we evidently not only require constancy in our chromosomes in order to account for constancy of characterization in a race, but we require that the *whole cell* should be constant. And yet it is because of the careful division and persistent shape, number, etc., of the chromosomes that the chief responsibility for the constancy of characterization has been placed upon them. If now the whole cell is required to be constant, what becomes of the special importance attached to mitotic division? There is yet another difficulty already referred to. The 'operations' of individual cells are not sufficient to account for the regular appearance of, say, a patch of pigment in a particular spatial relation to the whole, since this involves hosts of cells, and hence their mutual relations and their relation as a host to the rest of the organism.

Let us turn now to consider what is said about the hypothetical particles or 'genes' which are believed to constitute the chromosomes and to be strung along them like beads on a thread. We require to know what properties geneticists and cytologists assign to them. Prof. Wilson writes:

> 'The essential fact, from which the genetic evidence seems to leave no escape, is that they are self-perpetuating, and must, in some fashion or other, preserve their identity from one generation of cells to another.'[1]

He also says on the same page that it is 'convenient to think' of them as:

> 'analogous to the plastids or centrioles of the cytosome and capable of division, but lying for the most part beyond the reach of direct microscopical vision.'

The following passage from Prof. McClung is also interesting

> 'If the chromosomes are such definite and characteristic structures as the theory of their function demands, there must exist a process of incredible exactness for their perpetuation. Groups of organisms whose kind have been in existence for millions of years present to-day, in every cell of every individual, exactly the same visible complement of chromosomes. During this time there have been sequences of changes in inorganic matter so extensive as to produce whole series of chemical elements from one, and yet through all the manifold and apparently unstable conditions of organic existence the chromosomes of known systematic groups present an appearance of fixity and stability that is marvellous.'[2]

[1] *The Cell*, p. 1111. [2] *General Cytology*, p. 634.

It is interesting to contrast this, which refers to the chromosomes, with what Prof. McClung says (p. 612) about the chromatin—the 'hereditary substance':

> 'In the living condition it is not a solid substance moved about by extraneous forces, but a semi-fluid colloid, capable of a wide range of intrinsically controlled movements. These changes of form and position are obviously attended by liquefactions and gelations of a definite and limited order.'

With these points in mind I wish now to pass on to consider certain speculations about the immanent Mendelian factors which do not arise out of the genetical data and are not required by them, and which, moreover, appear to present difficulties which are worth pointing out. Comparisons are frequently made between Mendelian genes conceived as material particles and the hypothetical particles of physics and chemistry. Not only is the obvious methodological similarity noticed but the comparison is often carried still further to an ontological identification. Prof. T. H. Morgan's writings illustrate both these tendencies. On p. 1 of his *Theory of the Gene* he writes:

> 'In the same sense in which the chemist postulates invisible atoms and the physicist electrons, the student of heredity appeals to invisible elements called genes. The essential point of this [illegible] that both the chemist and the student of heredity— [illegible] from numerical and quantit[illegible]e data. The theories justify themselves [illegible] as they per[illegible] numerical and quantitative prediction of a specific kind. [illegible] essential respect the theory of the gene differs from [illegible] biologi[illegible] theories that have also postulated invisible units [illegible] arily assigned any desired properties. The theory of the gene reverses this order and derives the properties of the genes, so far as it assigns properties to them, from the numerical data alone.'

The earlier theories referred to were not concerned with Mendelian genetics but with development. Weismann's theory, for example, simply consisted in inventing a complicated architecture in the nucleus which was taken to pieces in development and the various bits were supposed to be parcelled out among the various cells resulting from division. The theory of the gene is not concerned with development but with genetical ratios. We must make a closer comparison between the biological and the physical theories. In the first place it must be noted that mass and energy in physics are not derived from the numerical data alone—indeed no

theory can work only with numerical data. It would be truer to say that some properties are 'arbitrarily assigned' to the physical particles. They are concepts—constructions in thought representing historically an immense amount of intellectual work—and constituting a refinement of every day concepts which are related to much the same facts of experience as those with which the physicist begins. As Prof. Cassirer points out, the scientific hypothesis does not come after the numerical data but *before* them:

> 'The "true hypothesis" signifies nothing else than a principle and a means of measurement itself. It does not appear *after* the phenomena are already arranged and known, in order to add to them subsequently a supposition about their absolute grounds, but renders possible this very arrangement itself; it does not jump over the sphere of fact in order to reach into a transcendent beyond, but marks out the way along which we reach from the sensuous manifold of sensations to the intellectual one of measurement and numbers. . . .'
>
> '. . . the demand of measurability . . . is quite right, but it is erroneous to regard measurement itself as a purely empirical procedure, which is completed in mere perception and with its means. . . . In fact it is evident that the mere experiment of measuring contains postulates which are never fulfilled within the sphere of our sense-impressions. We never measure sensations as such but always only objects to which we relate them. . . .'[1]

Returning now to our comparison it is especially noteworthy that the objects with which the chemist and physicist deal are supposed to be *composed* of the atoms or other particles, i.e. the perceptible thing is regarded as an aggregate of such particles—at least on the traditional view. But the object of biological study—the organism or race—is not supposed to be composed of genes. Thus the relation of the biologist's particles to that which they are to explain is not the same as the relation of the physical particle to that which it is intended to explain. In most cases what the physicist measures is regarded as the consequence of a statistical average of the doings of the particles all of which are supposed to be alike. In genetics on the other hand, we are supposed to be dealing with particles which are of very many different highly specific kinds in a given chromosome. The numerical data here primarily concern the number of individual organisms having a certain character in a given generation. The gene is designed to explain the distribution of the characters among such a generation of whole organisms. The genes are not composing

[1] *Substanzbegriff und Funktionsbegriff*, Berlin, 1910, p. 184.

units in an ordinary atomistic sense—not even the cell is composed of genes. Only the chromosomes are supposed to be composed of genes and the observed numerical relations between differently characterized organisms are interpreted as resulting from the occurrence of different genes at different places along these chromosomes. But the relation of these particles to that which they are intended to explain is admittedly unknown.

So much, then, for the differences between the biological particles and the physical ones from the methodological standpoint. Now Professor Morgan would like to go further and *identify* the hypothetical particles of genetics with the hypothetical particles of chemistry. He discusses the question whether genes are 'organic molecules'. Referring to the stability of the gene he writes:

> 'By stability we might mean only that the gene tends to vary about a definite mode, or we might mean that the gene is stable in the sense that an organic molecule is stable. The genetic problem would be simplified if we could establish the latter alternative. . . . There is little hope at present of settling the question.'[1]

On the same page he adds:

> 'When all this is given due weight it nevertheless is difficult to resist the fascinating assumption that the gene is constant because it represents an organic chemical entity. This is the simplest assumption that one can make at present, and since this view is consistent with all that is known about the stability of the gene it seems, at least, a good working hypothesis.'

Professor Morgan seems to have neglected Whitehead's maxim: 'Seek simplicity and distrust it'. Such an hypothesis is certainly simple but if it cannot be tested experimentally (and as there are supposed to be many *different* genes in *each* chromosome it obviously cannot) it can hardly be called a good *working* one. The only way in which such an hypothesis can be tested is by its consilience with the rest of knowledge. Let us therefore apply this test. Now since both chemical molecules and genes are hypothetical entities with which no one claims to be directly acquainted they are only known by definition. Consequently, if the properties of the one by definition are inconsistent with the properties of the other then the two kinds of particles *cannot* be identified. Now a chemical molecule is by definition the smallest particle of a certain kind of stuff which can exist. If it is divided you no

[1] *Theory of the Gene*, p. 310.

longer have a particle with the same properties. A molecule of water is believed to have quite different properties from a molecule or atom of either oxygen or hydrogen. Now compare this with the genes. As Professor Wilson says, they must be 'capable of division' since they are 'self-perpetuating' and nevertheless 'preserve their identity from one generation of cells to another'. We find in fact that the properties attributed to them are little short of all the fundamental properties of living things. They seem to be living organisms in miniature. At all events they are capable of division in the biological sense into two parts with similar properties. This is admitted on all hands. If it is not admitted the hypothesis breaks down. Consequently the genes *cannot possibly* be identified with chemical molecules as understood and defined by chemists.

Some critical remarks of Bateson are worthy to be recalled in this connexion. With reference to colour inheritance he writes:

> 'When we consider more critically it becomes evident that the aid given by this mental picture is of very doubtful reality, for even if it were true that any predestined particle actually corresponding with the pigment-forming materials is definitely passed on from germ to germ, yet the power of increase which must be attributed to it remains so incomprehensible that the mystery is hardly illuminated.'[1]

A little later he adds:

> 'When however we pass from the substantive to the adjective characters the conception that the character depends on the possession by the germ of a particle of a specific material becomes even less plausible.'[2]

Similarly, speaking of factors in his celebrated addresses at the British Association at Melbourne in 1914, he expressed the view that it is

> 'unlikely that they are in any simple or literal sense material particles. I suspect rather that their properties depend on some phenomena of arrangement.'

Certain other requirements of the particle hypothesis are also perhaps worth pointing out. If the gene is a material particle as ordinarily understood then, it seems, it cannot be smaller in width than the thickness of a chromosome at the time of longitudinal splitting. Because the hypothesis requires that

[1] *Problems of Genetics*, p. 34. [2] *Ibid.*, p. 33.

each gene should be exactly divided at each division (at least of those cells which furnish germ-cells). Consequently if the genes were thinner than the chromosome they would not be divided and might pass as wholes into one cell or the other, and the cells would not then have chromosomes of similar genetic constitution. This requirement also seems to be contrary to the hypothesis that they are single molecules. But might they not consist of groups of molecules? If this assumption is correct it would still seem necessary to suppose that they possess, and are themselves relata in, an organization of great complexity and extraordinary rigidity. Moreover, they have the property of answering the 'roll-call' at each division in a remarkable way—falling into their right places with great precision, unless we suppose that they are in such a position throughout the history of the cell. But this is a little difficult to reconcile with the statement that the 'chromatin' is a 'semi-fluid colloid, capable of a wide range of intrinsically controlled movements' which changes are 'obviously attended by liquefactions and gelations of a definite and limited order'. Perhaps there is no insuperable difficulty here, but the evidence in favour of the persistence of the chromosomes as such during the so-called resting phases of the nucleus does not seem to be very extensive or convincing to an unsympathetic critic. Nevertheless between two divisions of a cell each gene is required to prepare itself for division into two parts each of which has the same great specificity, which is different from that of every other gene in the mature germ cell, as well as to take its place prior to the next division in the nuclear hierarchy. If to these feats we add the still more remarkable rôle it is required to play in relation to the characterization of the developing embryo it must be admitted that the gene is a very remarkable entity. If the genes are to be conceived chemically one would hardly expect their chemistry to be very simple, and yet a physiologist has said that 'the nucleus . . . possesses a chemical constitution of no very great degree of complexity; so that we may even hope some day to see the material which composes it prepared synthetically'. He also adds:

> 'And when we consider that the nucleus . . . is in fact, the directing agent in all the principal chemical changes which take place within the living cell, it must be admitted that we are a long step forward in our knowledge of the chemical basis of life.

> That it is the *form* of nuclear matter rather than its chemical and molecular structure which is the important factor in nuclear activity cannot be supposed."[1]

Biological problems always present themselves to physiologists with a childish simplicity which is almost touching. If the chemistry of the nucleus is so simple, is this because of the crudity of the chemist's technique, or is the geneticist mistaken? In the present case it would be extremely unsafe to attach too much importance to the utterances of physiology because it is perfectly obvious that if the particles of the chromosomes were chemically different a chemical analysis of a quantity of mashed up cells would hardly be expected to discover this fact.

But we must not allow these speculations to lead us too far from the main point which is the antithesis between preformation and epigenesis. The present position in regard to the chromosomes seems to be that they offer a model for the interpretation of Mendelian ratios, but the genetical theories appear to have no point of contact with the problems of the embryologist. If the geneticists state their theories in a purely conceptual form—the so-called chromosome maps being regarded as expressing certain abstract relations within the organized system upon which the characterization of the organism depends, they remain in a perfectly firm position. But nothing seems to be gained, at least from the embryological standpoint, by expressing such a conceptual scheme by means of an imaginable picture. Such a picture has undoubtedly been of great heuristic value as is so often the case, but it seems to add nothing to biological knowledge at present which is of any avail for the interpretation of development.[2]

II

It is time now to consider the bearing of all these modern developments on the old antithesis between preformation and epigenesis. We can recall the opinion quoted by Prof. Wilson that 'fundamentally epigenesis is inconceivable' because it means that:

[1] Sir E. Schafer, Pres. Address Brit. Assn., 1912. Note 'directing agent' again.

[2] For a recent criticism of the chromosome theory see V. Haecker, *Phänogenetische Probleme*, Jena, 1913, especially pp. 130-67.

> 'we are unable to conceive how a self-determining system can increase its own initial complexity by interaction of its chemical and physical components. In so far as such a system is independent of external causes it can only transform and redistribute components that are inherent in the system from the beginning.'

This whole passage seems to be founded on a very a priori way of proceeding, and is illustrative of the habit which biologists have fallen into of putting too much reliance upon reasoning of this type instead of keeping closely to what is observed. Neither is it at all clear from the above what precisely is meant by saying that epigenesis is 'fundamentally inconceivable'. What we are or are not able to conceive, as that term is commonly used, has nothing to do with what is the case, and it is the business of science to find out what is the case. As Professor Whitehead says:

> 'Things are what they are; and it is useless to disguise the fact that "what things are" is often very difficult for our intellects to follow. It is a mere evasion of the ultimate problems to shirk such obstacles.'[1]

It is by no means clear what Professor Wilson means by a 'self-determining system'. If this means something which is independent of everything else in the universe it is doubtful whether anything of the kind exists, and it is still more doubtful whether it would be possible to establish the fact that this was its nature if we did find such a thing, because you cannot abolish 'everything else' in order to see what happens to the entity in question. The radio-active elements seem to be remarkably independent entities but developing embryos are not of this nature. Then again it may well be doubted whether the expression 'increase in complexity' is a suitable way of expressing what is observed in development. It is often stated that organisms are just complicated physico-chemical 'reactions' and it is because they are complicated that biology has so far made so little progress. But it is evidently not *simply* a question of complication because there are plenty of complicated goings on in the world which no one would mistake for organisms. The 'something going on' we call a thunder storm, for example, is very complicated. An organism from some points of view is comparatively simple, otherwise biology would not have got as far as it has, and this simplicity appears to be the outcome of its organization.

[1] *Concepts of Nature*, p. 119.

What happens in development is not merely an increase in complexity, nor an increase in structure, if by that you mean spatial structure alone. What we do observe, as I have tried to explain, is a gradual rise in the *level of organization* of the developing organism, and this appears to be beyond reasonable doubt an epigenetic process. Whatever may be the case regarding its 'chemical and physical components' the organism certainly increases its organization through that most pervasive of vital modes of change which we call cell-division. This is true apart from the question whether the organism is 'independent of external causes' or whether it 'can only transform and redistribute components that are inherent in the system from the beginning'. Because when an ovum has divided you have two *cells* (not two half-cells!) whereas before you had only one, and you not only have two cells but you have *in the whole* two *parts* not previously present as such since they *each* have the same level of organization as the original whole, and you not only thus have two new parts but *also their new relations*. Consequently the whole now has a new mode of organization, *above* what it has in the preceding slices of its history. But of course if you ignore organization and deny internal relations it becomes very difficult to see how there can be organisms at all, and epigenesis may well be 'inconceivable' under such conditions. If by epigenesis you mean a process in which the later slices contain more than the earlier, then development is certainly epigenetic, and the 'more' means more organization. And the organization of the later slices is in no sense 'contained in' the earlier ones. All that was contained in the fertilized ovum was the cell-type of organization and this is contained in all the later slices but as being the level of organization of certain parts (cells) not of the whole. The whole process rests at bottom on the spatial repeatability of this type of organization, although as we have already seen this is not the only mode of elaboration in virtue of which the level of organization may be raised. Such elaboration is of two fundamental kinds: either production of parts resulting in the establishment of a new and higher level, e.g. passage from cell-level to cellular level; or, elaboration of *different* parts of a given level, e.g. inter-cellular elaboration (elaboration of gut tube or neural tube) or intracellular elaboration (e.g. elaboration of myofibrillae). But it is needless to repeat these important concepts since

they have already been explained. It seems perfectly plain that it is impossible to describe the developmental process adequately if time and organization are not taken seriously. The problems of experimental embryology fall under four fundamental questions :

(1) Upon what is division dependent ?
(2) Upon what is the plane of division dependent ?
(3) Upon what is the formation of cellular parts dependent ?
(4) Upon what is intracellular elaboration in a given cellular part dependent ?

It might be desirable to exclude the first question from embryology on the ground that the cell-type of organisation and its fundamental property of spatial repeatability is the irreducible minimum from which the embryologist works. Anything less than an entity having this level of organization is not found alive in nature, if we except the bacteria. It may be that anything less than this is purely a matter for physics and chemistry and does not come within the province of biology at all. The chief objection to this possibility seems to be that the chromosomes and the hypothetical genes are also capable of division in the biological sense, as well, apparently, as other cell-parts, and hence have an organization above the chemical level. These considerations offer us a new definition of biology. If we construct a suitable clause to embrace the somewhat peculiar condition of the bacteria we may say :

> Biology is that branch of natural science which studies events that are knowable as single individual things exhibiting either as wholes or in their parts the cell-level of organization.

This definition has the merit of emphasizing organization and of avoiding the vague expression ' living things '. It also serves to differentiate biology from astronomy ! For according to Prof. Whitehead biology is the study of the larger organisms and according to his use of the term the solar system is an organism. The above definition has the further merit of not forgetting time, and of emphasizing the fact that the science is *primarily* concerned with single individual entities, not aggregates. And it seems probable that so long as there are such entities biology will be a ' selbständige Wissenschaft ' in the same sense in which every other science is ' selbständig '.

So much, then, for epigenesis ; we now have to consider

'preformation'. This term is of course a relic from former days and there can no longer be any question of *pre-formation* in any literal sense. Even the most ardent admirers of the chromosomes do not now appear to regard them as pre-formations with reference to development, although of course in a sense they are preformed and remain so throughout. But the point is that their preformation as such is no longer regarded as offering an explanation of development.[1] But if there is no longer preformation may there not be pre-something else? We now have to consider what that something else might be. Consider the following case: no biologist would be shocked or even surprised if a hen's egg in an incubator failed to develop. Such things occur only too commonly. But every biologist would be mightily surprised if, after its appointed time, the egg hatched forth a dragon, a dog, or even a duck, assuming it had in the first instance been duly observed to issue from a fowl, and assuming that there was no question about its paternal parentage. This brings us back to the question of characterization. It is not the development—the future parts—which are 'performed' but the *characterization* of those parts if and when they are realized. If we think of all the future slices of the history as 'represented' in the earlier slices we merely stuff the expected adult hen back into the egg as the preformationists did of old. But taking time seriously and the observed facts of development forbid us to do this. The characterization of a given slice on the other hand is in a sense non-temporal since it characterizes many different examples in the same race whether belonging to the same or to other generations. But a given slice of a given individual history, on the other hand, only exhibits a given set of characters. Accordingly the complex of 'immanent factors'—that upon which the characterization depends—represents a means of limitation or selection from a number of possible modes of characterization. And this complex of factors seems to be a persistent property of that which exhibits the cell-type of organization since this type of organization persists throughout the history pervading either the whole or its parts. This complex is immanent in every part which has that level of organization, and this enables us to understand the significance of equal division and the theoretical possibility of every such part

[1] Cf. T. H. Morgan, *Experimental Embryology*, New York, 1927, p. 8.

becoming a whole—a possibility which is realized in some organisms and is also witnessed in a different form in restitutive processes.

We must bear in mind what was said in an earlier chapter about modes of persistence. In studying development we are studying one of the modes of persistence of living organisms. Crystals persist by temporal repetition of their mode of organization. Living organisms persist in virtue of the spatial repeatability of the cell-type of organization. Thus whole-producing division in protozoa and metazoa is a mode of persistence; part-producing division in metazoa is also a mode of persistence. And the same applies of course to plants. Now if such division were unequal, persistence would not be achieved, and reproduction would not result in persistence. Hence division (spatial) as a means of persistence presupposes *equality* of division. But part-producing division, and other modes of elaboration also involved in persistence, involve *inequality*, which conflicts with the other requirements of persistence. These two requirements will both be satisfied if (1) some parts undergo elaboration *after* division; and (2) if some parts never undergo inequalizing elaboration. If this is the case we can understand why division is fundamentally always equal as seems to be the case, at least so far as the nucleus is concerned, and why elaboration results largely from the different mutual spatio-temporal relations of the parts, and not from unequal divisions, and also why persistence can only be achieved *through* change and development is an epigenetic process. The cells of an adult organism fall into three groups: (1) those which undergo intracellular elaboration to yield the parts exhibiting the adult characterization; (2) those which are still capable of division and exhibit no intracellular elaboration (mesenchyme cells capable of restitutive processes[1]); and (3) cells capable of being separated as new wholes and which may undergo intracellular elaborations in correlation with this (germ-cells). In considering development three fundamentally different questions should be carefully distinguished:

[1] Recent experiments do not support the old view that in regeneration the new tissues simply grow from the old ones. See also Prof. Harrison's experiments on mesenchyme cells in connexion with hæmopoiesis. *Physiol. Reviews*, Vol. IV, 1924.

(1) The question of the coming into being of the successive temporal parts of events, which is the problem of the nature of time and does not belong to biology as such.

(2) The question of the gradual elaboration of the organization of the history which is known as the enduring organism. This problem is the task of the embryologist, whose business it is to investigate the mutual dependence of the parts upon one another and upon the environment.

(3) The question of the characterization of those parts, which is the geneticist's problem.

Thus we are brought back to the distinction between parts and characters which was discussed in Section 8. There seems to be little doubt that it is here that we find the roots of the antithesis between epigenesis and preformation. Just as the antithesis between structure and function rested on the separation of space and time, so this antithesis seems to be expressive of the difference between events which pass and their characters which endure (' objects ' in Whitehead's sense). But this statement is too general as it stands to help us to clarify the particular form this puzzle takes in relation to biological problems. Prof. Whitehead states the general antithesis very clearly as follows :

> ' There are two sides to nature, as it were, antagonistic the one to the other, and yet each essential. The one side is development in creative advance, the essential becomingness of nature. The other side is the permanence of things, the fact that nature can be recognized. Thus nature is always a newness relating objects which are neither new nor old.'[1]

Now the difficulty which presents itself in the biological problem seems to be this : It is perfectly plain that characters in the ordinary geneticist's sense belong to the ' permanences ' in nature and that the immanent Mendelian factors have to do with the control of their manifestation. But it is equally plain that in the extended use of the term ' character ' modes of development and modes of organization are also permanences. Some types of organization (the cell-type) do not develop, but persist by spatial repetition. It seems therefore that these also should be dependent upon the immanent factors and be equally expressive of the immanent endowment of a given organic race. But such modes of

[1] *Principles of Natural Knowledge*, 1925, p. 98. But if the doctrine of evolution has any truth in it, and if evolution has been an epigenetic process then it seems that some objects will have been ' new ']

development and organization are often extremely pervasive and hence are called class and order characters. They appear to be very deeply rooted and not liable to 'mutation'. Now the difficulty is to harmonize this attitude with the facts of restitution and transplantation and with the epigenetic character of development. The difficulties presented by the experiments mentioned, in which transplanted parts develop 'ortsgemäss', seem to be overcome when we remember the differences between part and whole and when we admit that a given part, provided it has the cell-type of organization as a minimum, has immanent in it the whole genetic endowment. Now the characterization of *wholes* is not subject to *organic* regulation because their environment is inorganic and they are not parts of this in an organic relation. But *parts*, just because they *are* parts in organic relations, are capable of such regulation arising from their mutual relations in the whole, until a stage is reached when certain of their possibilities have become actual in accordance with their relations in the whole, and other modes of elaboration set in. In some organisms even this 'stagnation' can be overcome apparently by 'dedifferentiation'. Now development is epigenetic on account of the peculiar way in which the organic parts are produced. Development is a process in which with temporal passage new spatial parts come into being all with the same genetic endowment. As development proceeds they do not *lose* 'factors' but selection is made from their possibilities and this depends on their mutual relations, on the relation of the whole to the environment, as well as on their immanent endowment. It is not the parts and the coming into being of parts but how they are characterized which depends on genetic factors. It is because of the pervasiveness of the earlier modes of development that an organic part from an embryo of one species can be transplanted to a different place in another embryo of a different species. And it is because of this pervasiveness that they do not admit of analysis by Mendelian crossing, but they are not any the less dependent upon immanent factors.

I conclude then that all permanences discoverable in development are expressive of enduring properties of complexes of events when they reach the biological level of organization, and it is these enduring properties that are dependent upon immanent factors. In the course of

development part-events are elaborated whose characterization depends partly on this immanent endowment and partly on their mutual relations to other part-events. There can be little doubt that some of these immanent factors are intimately linked with the events known as chromosomes, and that they have an 'atomic' character. But this atomicity is also in some way overcome by the organization of the whole since it is the characterization of the whole which depends on them and this cannot be interpreted as the outcome of the doings of unrelated cells. The concept of organization requires organic *relations* as well as relata. If we forget this we shall be driven to have recourse to imported entities to make our aggregate into an organism. Thus the embryologist is concerned with the mutual relations of the parts and of the whole to the environment during the process of realization of a given limited immanent endowment, which he takes for granted. And this is limited as regards the whole, but not limited as regards the parts within the limitation of the whole, but the limitation of the parts is itself only completed (save for mesenchyme and germ-cells) during the process of realization itself. The geneticist, on the other hand, is not concerned at all with this process of realization but with the characterization of what is realized in a given history as a segment in a genetic succession of histories.

Summary.

I will now try to summarize the whole of this long and complicated discussion of the problem of preformation and epigenesis as it is presented by the process of development in the individual.

Let us begin by stating the causal postulate in the form in which it is employed in regard to embryological problems:

> If a given organism during a period of its history is different from what it was during a previous period, then the difference between these periods is traceable (at least theoretically) either to changes going on within the organism between the two periods, or to changes in the environment between or during the two periods, or to changes in both.

This is clearly the way in which the causal postulate is used by the embryologist, since the developing embryo is such that no two periods of its history are the same, and the problem of the embryologist is to explain the differences (cf. p. 336).

The following is another way of stating the causal postulate when it is applied, not to different periods in the history of the *same* organism, but to the differences between two organisms belonging to the same natural kind.

> If a given organism differs from another organism belonging to the same natural kind (in the sense that it is *known* to be genetically derived from it, or genetically derived from an organism from which the other organism is also genetically derived) then the differences between these two organisms is traceable (at least theoretically) either to immanent differences between the two organisms, or to differences in the environments contemporaneous with them.

This is clearly the way in which the causal postulate is used by the geneticist, since no two organisms belonging to the same natural kind (in the above sense) are only numerically different (except perhaps in the case of 'identical' twins), and the problem of the geneticist is to explain the differences. If all organisms belonging to the same natural kind differed only numerically there would be no genetical problem.

But if (1) the developing embryo is such that no two periods of its history are the same, and (2) no two organisms belonging to the same natural kind are only numerically different, how is embryology possible in accordance with the causal postulate? For if (1) is true, a given change cannot be repeated in the same organism, and if (2) is true, it cannot be repeated in another organism of the same natural kind since this will not merely differ numerically from the first organism. The obvious answer is that two organisms belonging to the same natural kind, and developing in similar environments, will only differ from one another in features which are of little or no importance for embryology in its present state of development.

Now let a, b, c, d, . . . , x, and a', b', c', d', . . . x' represent two organisms, developing in the same environment E, and considered four-dimensionally, i.e. a and b and c etc. represent different temporal parts of the same organism. For example, a represents the period in the history which is known as the fertilized ovum, b the two-celled stage, c the blastula, and d the gastrula, and so on to the adult x. Then the genetical problem is to explain the difference between one history as a whole and the other as a whole or between a part of one and a corresponding part of another. The fact that geneticists mostly work with the difference between a and x' is of

practical rather than theoretical importance. The embryological problem, on the other hand, is the interpretation of the difference between *a* and *b*, or between any two temporal parts.

Let it be supposed that the environment is uniform with respect to both organisms throughout the developmental period. Then if *x* and *x'* are perceptibly different, even if *a* and *a'* are not perceptibly different, the causal postulate directs us to assume that *a . . . x* and *a' . . . x'* have throughout been intrinsically different, since *ex hypothesi*, the environment has been uniform. In other words a geneticist would postulate different 'genetic factors' in the two cases.

Now consider the difference between *a* and *z*, the embryological problem. If the environment is uniform throughout the history with respect to *a . . . z* in the sense that any changes in it are not such as to be followed by changes in *a . . . z* which would not otherwise have occurred, then, assuming the causal postulate, the difference between *a* and *z* can only be explained by assuming that intrinsic or immanent changes take place in *a*. And if the changes which take place in *a* are different from those which take place in *a'*, then *z* will be different from *z'*. We now have to consider the different possible factors in virtue of which *z* finally comes to differ from *a*, assuming *E* to be uniform throughout the history, so that we cannot appeal to differences in it to account for differences in *a . . . z*. The difference between *a* and *b* comes into being through the process of cleavage. The two non-cellular parts which result from this process may have either (1) the same intrinsic properties, or (2) different intrinsic properties. Theories of the Weismann type supposed that the latter alternative was true. But experiments show that in many cases the first alternative is true. And if we adopt this alternative we can only explain the difference in the behaviour of the two cells of *b* according to whether they are or are not related as parts of one whole (i.e. allowed to remain in their normal relation or separated), by ascribing it to the difference in their mutual relations in the two cases. In other words, the relation described as 'being parts of one whole' (whatever further analysis it may be capable of) is an internal organizing relation, since the parts behave differently in this relation from what they do

out of it. But the two cells of *b* do not persist as such, since each divides again and so on to *c*—the blastula, and similar internal organizing relations hold between the cells of the blastula as held between the two cells of *b*, but they will be more numerous and complicated. If this is so, one cell may differ from another cell not intrinsically but in virtue of its relations to the others. But in passing from *c* to *d* we have a different problem, namely to account for the elaboration of two *cellular* parts. If *c* exhibited some sort of polarity, as is usually the case, this would enable us to understand the next step since this original polarity will now be maintained in differences between the cells of one pole as a whole and those of the other, even though this be only a difference in size, as appears to be the case, for example, in *Amphioxus*. When gastrulation is accomplished in virtue of these differences it is not difficult to see that there will be plenty of scope for further differentiation in virtue of differences of the mutual relations between the parts both cellular and non-cellular. The mutual relations of the cells surrounding the blastopore will be different from those within the archenteron, both between themselves and towards the environment. A further difficulty is presented by the problem of bilateral symmetry. To explain this we may either appeal to the properties of spatial relations or, if this is considered too heroic, to an original bilaterality coupled with the original polarity. But when once this is established we know from Spemann's experiments that internal relations and not intrinsic properties of the cells are of importance for further elaboration.

But even if we are able thus far to interpret development by appealing to only a minimum of intrinsic differences between the cellular and non-cellular parts, a time comes when perceptible differences make their appearance between the cells, i.e. when intracellular elaboration sets in. Some mesenchyme cells elaborate myofibrillae, others (e.g. in vertebrates) elaborate haemoglobin, and so on. Here again mutual relations are doubtless still important, but whereas hitherto we have largely been concerned with modes of division of (possibly) equivalent cells, how will such relations help us to understand the development *within* a given cell of myofibrillae or glandular secretion, or calcareous salts? Let us suppose that *every* cell is capable of developing myofibrillae, that

every cell is capable of developing haemoglobin, etc., in other words that every cell is capable, up to a certain period in the history, of becoming *any* adult fully elaborated cell of an organism *belonging to that particular species*. For at a certain period the cells stop dividing and begin their intracellular elaboration. Why or how this happens nobody knows, but it may depend on temporal relations. But granting that it does set in at a certain period (which is a fact), and granting that every cell is capable, in virtue of its *intrinsic* properties, of differentiating into any elaborated cell of its species (which is a hypothesis), then we could understand that *which* of these possibilities was realized—which particular elaboration appeared—might depend again upon the internal organizing relations between the cellular and non-cellular parts, just as other developmental processes appear to do.

In this way, then, in broad outline, we could understand how the difference between *a* and *z* becomes established in a uniform environment, by appealing to intrinsic properties of the cells, and to differences between them which arise from the different mutual relations which come into being with the parts which stand in them, these being the result of either equal or unequal divisions. And it is evident that if the successive steps in *a'* . . . *z'* differ at any period from those of *a* . . . *z* then *z'* will differ from *z*, and this will be traceable, at least theoretically, to intrinsic differences between *a* and *a'*. Moreover, the intracellular elaborations when we get to *z* depend, according to hypothesis, on intrinsic properties of the cells constituting (with their relations) the temporal part *z*. But these *z* cells result from the repeated divisions of *a*, and if *a* differs intrinsically from *a'* the *z* cells will differ intrinsically from the *z'* cells, and hence the intracellular elaborations will differ in the two cases, *e.g.* the pigment of the frog will differ from that of the newt. This explains how it may be (as is the case with some species) that if a group of cells from an embryo of one species at an early period of development is transplanted to some region of the embryo of another species, it may be capable of development in its new site since the latter provides an organic environment for it. Moreover, since the rôle of such cells during the early periods depends on their relations to other parts in the whole (these being internal relations) we can understand how it is that such transplanted parts develop according

to their new situation (ortsgemäss) and not according to their place of origin (herkunftsgemäss). But now, such transplanted parts, when they are fully elaborated, exhibit the full specificity of characterization according to the species from which they have been derived. And since this depends (according to our hypothesis) upon the *intrinsic* properties of the cells, since only *which* of these properties is realized depends on relations to other parts, we can also understand by means of this hypothesis these apparently paradoxical facts of transplantation.

It is not difficult to bring this account of development into relation with the requirements of genetical data. In all cases where we appeal to intrinsic properties of cells, whether it be the fertilized ovum, or the period of intracellular elaboration, we have to do with properties depending on genetical factors. But wherever we have changes dependent upon internal organizing relations between the parts then we are dealing with shifting epigenetic developmental processes. That the nucleus is of great importance in regard to intrinsic properties there can be little reasonable doubt, and if all somatic divisions are equal we can understand that up to the period of intracellular elaboration the cells may all be equivalent in their intrinsic properties, and that which of these are manifested depends on their mutual relations. Cells which do not undergo such elaboration may persist as mesenchymatous cells capable of regenerative processes, again in accordance with their position in the whole. Thus all that is 'preformed' (if we are to continue to use that unsuitable term) is the number and nature of possible modes of intracellular elaboration which a cell may undergo in a given individual of a given species, and also certain developmental processes which depend on the intrinsic properties of the ovum, e.g. cleavage pattern. Within those limits the rest is 'epigenetic'.

In this sketch of development I have made no reference to the so-called 'mosaic eggs' not because I wish to ignore them but because they appear to present much less difficulty. The most difficult cases are those which present the minimum of 'cytoplasmic preformation'. If we can understand the most difficult cases the others can easily be brought into relation with them.

My view, then, regarding individual development is that it ought to be possible to interpret this in accordance with the

causal postulate in terms of the intrinsic properties of cells and cellular parts *and their relations* if we can admit the following propositions:

(1) An entity having the cell-type of organization is such that it is capable of repeated division in the biological sense without loss of intrinsic properties.

(2) The cells of a given developing embryo are internally related to one another in the sense that the rate and plane of division, at least, of a given cell, depend on its relations to the neighbouring cells, and hence on its position in the whole.

(3) Every cell in a given embryo is capable (at least in the most general case) of undergoing any of the intracellular elaborations characteristic of the adult cells of the species to which it belongs.

(4) At a certain period of development the cells of an embryo undergo intracellular elaboration depending partly on their mutual relations, and partly on their intrinsic properties, so that although the latter may be the same (in virtue of equal nuclear divisions) in all cells, which of them are realized in a given cell will depend on its relations in the whole, which will of course differ from place to place.

The general problem of the antithesis between preformation and epigenesis will be discussed after evolutionary development has been dealt with.

By way of appendix to Division I of this chapter I wish to draw attention to certain easily avoidable confusions still extant in the terminology of genetical topics which are serious in so far as they also lead to, or are the outcome of, confusions of thought. And first it is desirable to consider the meaning and use of the term 'heredity'. From the very diverse definitions of this word given in biological literature it seems clear that the word stands for an indefinable abstraction, and in the interests of precision it could be banished from scientific terminology with advantage. It is a vague term borrowed uncritically from common sense. It does not appear to be *used* in any of the senses given and consequently it is calculated to do nothing but breed confusion. I will give a number of examples:

'We may define heredity as an innate capacity of the organism to develop ancestral traits.'—E. B. Wilson, *The Cell*, p. 1057.

'Heredity may be defined as organic resemblance based on descent.'—W. E. Castle.[1]

'Heredity is commonly defined as the tendency of offspring to develop characters like those of their parents.'—Babcock and Clausen.[1]

[1] Quoted in J. A. Thomson's *Heredity*.

'the organic or genetic relation between successive generations.'—J. A. Thomson, *Heredity*, p. 13.

'The transference of similar characters from one generation of organisms to another, a process effected by means of the germ-cells or gametes.'—R. H. Lock, *Rec. Prog. Stud. Variation*, etc., p. 192.

'Heredity is the law which accounts for the change of type between parent and offspring, i.e. the progression from the racial towards the parental type.'—K. Pearson, *Grammar of Science*, 1900, p. 474.

'Under heredity we understand the transference to the offspring of qualities of the parent or parents.'—T. H. Montgomery, J. Amer. Phil. Soc. xliii, 1904, p. 5.

It is perfectly evident that what these various authors are referring to by the word heredity is by no means the same in all cases, but what precisely any particular one is referring to is by no means easy to discover. Prof. McClung very justly remarks that:

'It appears from definitions variously given that heredity is at once a "law", "rule", "force", "material contribution", "act", "relation", "process", "fact", "principle", "link", and "organization". Little wonder that discussions of the subject are so lacking in clearness and precision when the central conception is so poorly defined. An outstanding instance of this confusion of ideas is the almost universal practice of contrasting heredity and variation as factors in evolution. Since variation is only a measure of the precision of hereditary processes, the subject is merely obscured by conceiving them to be opposing forces. Difficulties are introduced also by inaccurate statements which seem to place the operation of hereditary forces in a position, if not opposed to, at least independent of environmental conditions.'[1]

With the first part of this passage I am in hearty agreement, and it is to be regretted that more biologists do not pay a little more attention to this side of their work. But I doubt the value of such expressions as 'hereditary forces' and the simplest way of dealing with the term 'heredity' is to abolish it altogether. I do not feel that Prof. McClung improves at all on the definitions he criticises when he offers the following:

'Heredity then is the condition or state which is maintained by a certain balance between the operation of forces or conditions intrinsic in a given protoplasmic organization and the external conditions within which it is placed. It also is continuous and a function primarily of the group and not of the individual.'[1]

The fact that these definitions are not in fact employed is

[1] *General Cytology*, pp. 613-4.

easily seen if we substitute the definitions in propositions in which the word occurs. We then find that the result is either nonsense or not what the author intends. Prof. Wilson, for example, says :

> 'The conclusion is irresistible that normal development . . . depends upon a normal combination of chromosomes. And since heredity is the product of development it follows that each chromosome must play a particular part in its determination.'[1]

Now substitute Professor Wilson's definition of heredity in the last sentence and we have :

> And since an innate capacity of the organism to develop ancestral traits is the product of development it follows that each chromosome must play a particular part in its determination.

And it is clear at once that the term heredity is not being used in this passage in the sense in which Professor Wilson defines it, for no one contends that an innate capacity of the organism to develop ancestral traits is the *product* of embryonic development. But if this is not what Professor Wilson means by heredity in the above passage what *does* he mean ? This appears to be a good example of the pit-falls we are exposed to by the use of indefinable abstract substantives. Such terms are admissible enough in common speech where quickness rather than precision is required but it is fatal to admit them into serious scientific discussion. What we find in nature and what the geneticist deals with are characterized organisms known (in genetics) to be related to one another by the genetic relation and so constituting a race. A given character of a given spatio-temporal part of a given individual stands in a three-termed relation to factors immanent in that individual and to its environment. All these terms are perfectly definite and nothing seems to be gained by introducing the term heredity. Some of the definitions given above are obviously very bad, particularly those which speak of the transference of characters from parent to offspring. This mode of expression seems to be nothing more than a derivative of the legal use of the notion of inheritance, but enough has been said to show the uselessness of trying to patch up such definitions, when the term itself is not required for scientific purposes.

[1] *The Cell*, p. 913.

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Some further conclusions relating to this subject are well illustrated by the remarks of Mr. J. T. Cunningham in his book *Modern Biology* in which he attempts to criticize some current notions in genetics, and in particular the elementary exposition of them given by Prof. E. S. Goodrich in a little book called *Living Organisms*. Prof. Goodrich's exposition certainly is not as clear as it might be but Mr. Cunningham's criticisms only serve to make matters worse. The view which Mr. Cunningham objects to is expressed in the following passage on p. 52 of Prof. Goodrich's book:

> 'Every character of whatever kind is, in a sense, partly acquired, since it is called forth by stimuli, and partly inherited, being a product of the activities of the germ-plasm; and no character is more acquired or more inherited than any other. Hence the popular distinction between acquired and not acquired characters, between those which have been developed in the course of the individual's life-time and those which are inborn, i

This seems to mean simply that every character manifested by a given organism depends partly on factors immanent or intrinsic to it and partly on environmental factors contemporary with its history. In the next paragraph Prof. Goodrich says:

> 'And yet there are characters ' offspring and others which do *not*; in are inherited and others are not '

This seems to be in flat contradiction to what was said above. The author therefore says that it is necessary to revise the 'popular conception of inheritance just as we have revised that of "acquired characters"'. He says that the word 'inherited' should mean 'that a character possessed by the ancestor reappears in the offspring'. Also he uses the term 'transmitted' and says that factors are transmitted and characters are inherited. But would it not be simpler to avoid these expressions entirely? If as seems to be the case every part of an organism which has at least the cell-type of organization has immanent in it the whole racial endowment (or in mixed races a selection from a number of possibilities) and since normally a new history begins as a separated part with the cell-type of organization (germ-cell) it seems simpler to say that the characterization during any period of that history

will depend partly on that endowment and partly on the contemporaneous environmental events. If variation occurs, i.e. if the offspring differs in respect of a given character from the parent, this is usually traceable either to environmental differences or assumed to be traceable to immanent differences. In the latter case according to current opinion this will be traceable either to a change in part of the immanent endowment itself (mutation) or to the fact that the endowment of the race is mixed and different combinations have resulted from amphimixis.

Prof. Goodrich's account of variation seems to be very muddled. He says he understands by this term :

> ' the extent or degree of difference between the characters of two individuals, or between an individual and the average of the species, the divergence of the new from the old : not a new character, but a difference which can be measured or at least estimated.'

He says that there are two quite distinct kinds of variation for which new names must be found. He then goes on to explain that he uses the term ' mutation ' for ' new characters due to changes in the factors of inheritance ' and the term ' modifications ' for ' those induced by changes in the environment '. Thus he begins by speaking of two kinds of *variation* by which he says he means the extent or degree of difference *between* the characters of two individuals and then proceeds to give the new names which he says must be found for these two kinds of variations to *new characters*.

Now let us turn to see what improvement Mr. Cunningham makes. On p. 100 of his book he writes :

> ' Different characters developed under the same conditions are not the resultant of inheritance on the one hand and the influence of the environment on the other.'

As an example he mentions duck's and hen's eggs both developing in the same incubator and adds :

> ' The oxygen, warmth, and the yolk in the eggs were necessary for the development of both kinds of eggs, but they had not an iota of influence in determining that one egg should become a duckling and the other a chick.' ' The special characters were entirely, not partially, due to the genetic factors, the external conditions only enabled them to show themselves in the fully developed young bird.'

I should of course prefer not to speak of a hen's egg becoming a chick but would say that it was an early temporal slice of

the fowl-history. But the most serious mistake is in saying that the 'special characters' of the two birds are 'entirely due to gametic factors'. It is perfectly plain that Mr. Cunningham is confusing two quite distinct and easily distinguishable questions. He is simply confusing the *differences* between the two birds with respect to some character with the different *characters* themselves. The *differences* between, say, the webbed feet of the one and the plain feet of the other may be described as 'entirely due to gametic factors'. But it is surely not possible to say that in the manifestation of webbed feet the oxygen and warmth had no concern, and Mr. Cunningham admits this himself when he says that such things are 'necessary for the development of both kinds of eggs'. The coming into being of parts knowable as webbed or plain feet is dependent upon a certain temperature, etc., but the webbedness of the one as contrasted with the plainness of the other—this *difference* is regarded as theoretically traceable to differences in the factors immanent in the two eggs.

On p. 101 Mr. Cunningham wishes to make a distinction between 'stimuli to the development of the organism' and 'stimuli to the production of any particular character'. As an instance of the former he mentions rise in temperature for the development of a fowl's egg, and of the latter he cites the case of sunlight on the human skin. He says that whereas a rise above atmospheric temperature is a 'necessary stimulus' to development for both an egg of a sparrow and that of an ostrich this is not the case with the frog's egg, nor is it essential in determining what species of frog the egg develops into.

This, with other passages, strongly suggests that Mr. Cunningham's troubles rest on erroneous notions about causation, and especially on an incautious use of the term 'stimulus'. If the above passage means that external circumstances have nothing to do with the characters manifested by a particular frog it seems to be plainly wrong. By calling a rise in temperature a stimulus to development all that is meant is that unless the temperature rises the egg does not develop. By calling sunlight a stimulus to the production of a particular skin-character what is meant is that in the absence of light of a certain kind the skin remains white and in its presence

it darkens. But in both cases one factor out of a complex is isolated and emphasized at the expense of the others, and it is only courting confusion to suppose that nothing else but sunlight or rise in temperature is required or involved and that a particular manifestation is solely dependent on such 'stimuli'. The sunburn is as much dependent on the skin as on the light. The skin of some people sunburns and that of others does not. Is there any fundamental difference between the development of the fowl's egg when the temperature rises and the appearance of sunburn when the intensity of the sunlight rises? In both cases we have a change in an organism correlated with a definite environmental change, and in both cases the particular nature of that change is partly dependent on the organism and partly on the environmental change. Precisely the same mistaken notion about causation underlies the following remarks of Mr. Cunningham. On p. 101 he says:

> 'Because characters as such are not present in the fertilised ovum, but only appear as the result of development, therefore it is said they are acquired. The proper term is "developed", and in biology the word acquired is applied to characters which are different from those due to normal development, and which are determined by special external stimuli, and absent when the stimuli are absent.'

Thus Mr. Cunningham wishes to make a sharp distinction between characters which are manifested at birth and those which are manifested afterwards, and he seems to believe that the former are solely dependent upon immanent factors and the latter solely dependent upon environmental factors. This is particularly clear from Mr. Cunningham's remarks about the development of cyclopian monsters in fish developing in abnormal sea-water. He says that 'the abnormalities produced are entirely due to the abnormal conditions applied if the genetic factors in the embryos under experiment are the same as in embryos which develop normally' and adds:

> 'It is agreed that the result shows that these genetic factors possessed the potentiality of producing the abnormal development observed under the experimental conditions, but this does not alter the fact that the special abnormality was determined by the experimental condition.'

These two passages show the difficulty in regard to causation with complete clearness and they flatly contradict one another.

If it is true that 'the result shows that these genetic factors possessed the potentiality of producing the abnormal development' then it is not true that 'the abnormalities produced are entirely due to the abnormal condition applied'. Either the genetic factors are irrelevant to the result or they are not irrelevant. If they *are* irrelevant what does it mean to say that they 'possessed the potentiality of producing the abnormal development?' If they are *not* irrelevant how can the result be *entirely* due to the abnormal conditions applied? It seems therefore that Mr. Cunningham has failed to maintain a distinction between characters which can be referred solely to the environment or solely to the immanent endowment of the organism, and I am unable to see how such a distinction could be maintained. But at the same time this fact does not appear to me to decide anything regarding the old quarrel between Darwinism and Lamarckism.

In all the above discussion we have not criticized the notion of 'immanent' 'intrinsic' or 'genetical' factors. We have assumed that in all cases the organism has intrinsic properties upon which in a given environment its characterization depends. We have discussed some of the difficulties attending attempts to conceive the nature of these factors in terms other than those immediately demanded by the data of genetics. But there are other difficulties which become apparent as soon as we try to harmonize evolutionary notions with genetical and embryological ones.

Division II. Racial Development.

14

It was said at the beginning that the antithesis between preformation and epigenesis arose in connexion with processes of development both racial and individual. We now come, therefore, to consider it in relation to racial development. This will involve a consideration of the related antithesis between Phylogeny and Physiology, or between the Historical and so-called Causal views of the organism. It will therefore be necessary to consider them together, and first to discuss the difference between historical and causal explanations.

We speak of individual development and evolutionary processes as two examples of development. What then have

they in common upon which this classification rests, and in what respects do they differ? In the case of the individual we saw that what developed was the type of organization which characterized the organism during different parts of its history. In the course of development the type of organization of the whole organism becomes progressively more elaborate—new parts and new relations between them gradually coming into existence. Moreover the serial changes through which this is accomplished can be observed in every member of the same species. It is a type of serial change which is characteristic of many events.

Now when we compare this with the evolutionary process we find that the latter is also a process in which there is a gradual (or it is believed to be gradual) appearance of types of organization of increasing elaborateness, except along those lines in which degenerative changes appear to have occurred. But when we compare the two cases we find a number of points of difference of fundamental importance. In the first place whereas individual development is a process which can be directly observed so that we can note the continuity of character between one slice and the next, this is not the case with evolution. The latter is a hypothetical process not a directly observed one. It seems to be the custom among biologists at the present day to speak of the *fact* of evolution, not of the theory. But this only means that the doctrine is now regarded as a hypothesis of a high degree of probability because it enables us to bring a great number of facts together which would otherwise be isolated, and nothing is known to contradict it.[1]

A second difference between these two instances of 'development' lies in the fact that whereas in the first case it was a process characterizing one individual thing, in the second case it was one characterizing races of organisms. It thus amounts to asserting that in certain cases the race has not exhibited a strictly rhythmical mode of characterization but a *serial* one.[2] That is to say—if we were able to follow back in detail certain past histories we should find that each 'unit' in the succession had not simply repeated the type of characterization of its predecessors, but had departed from it, and some succeeding

[1] Also there is no serious *rival* hypothesis.

[2] It never is of course *strictly* rhythmical owing to environmental contingencies.

ones had likewise departed from it, and in the same direction—leading say from reptiles to mammals. Thus evolution contemplates gradual elaboration of types of increasing elaborateness of organization not in a single organism but in a succession of genetically related organisms, and is thus a serial process just as individual development is a serial process. But, as we shall see, this must not be thought of simply as an increase in spatial structure in the ordinary sense.

A third difference is related to the one last mentioned. The evolutionary process, because it is serial and because it is accomplished in a genetic succession of increasingly different individuals, is *unique*. Every event is of course unique, and in that sense an individual organism considered as an event is unique. But in its case it was, as we saw, a part of a rhythmical succession, and consequently we can compare one individual development with another, and can recognize the same mode of characterization of a number of events. But in the case of evolution we cannot do this. The series of changes which have characterized the total event that we call evolution have, so far as we know, only occurred once. This fact has certain methodological consequences which we shall consider in a moment.

A fourth difference lies in the fact that whereas in the case of individual development we can be said to know a beginning, namely the fertilized ovum, in the case of evolution we do not know a beginning.

A fifth difference depends upon the difficulty of saying whether evolution is still going on. By this I mean that opinion is divided regarding the correct interpretation of the evolutionary significance of racial changes that are observed and studied at the present day. But all this is, of course, a matter of debate.

Of these five differences between individual and racial development the most important appears to be the second one, that, namely, depending on the fact that one is a type of change characterizing single individual organisms, and the other is a process of change in a genetic succession of individuals. It is therefore a change characterizing the 'secondary' biological 'substances'. The other differences are evidently dependent upon this one.

Now we can turn to the antithesis between Phylogeny and Physiology, i.e. the relation between so-called causal and

historical views of the organism. It is a prominent characteristic of present day biology that it has to a very large extent turned its attention away from the phylogenetic problems which occupied the centre of interest during the last century. This is partly a wholesome reaction against the highly speculative character of phylogeny, and partly a consequence of the extension of physiological methods. But it should not be forgotten that there *is* an historical aspect to organisms and that there are methodological problems connected therewith. We must therefore consider the difference between historical and what we may for the present call 'causal' explanations.

Historical and Causal Propositions and Explanations

There are two types of 'genetic' or 'historical' explanations—according to whether they refer to the individual or to the racial history. They are usually confined to anatomical data. For example the fortuitous course taken by a nerve or an artery may strike us as strange and we seek an explanation (i.e. for why it does not conform to our expectations) and we find one if it can be shown that in the course of individual development this state of affairs has been brought about by the shifting of neighbouring parts. This seems to be what is meant by an embryological explanation which is 'merely descriptive'. But it is more than a historical explanation because we can generalize it on the assumption that a similar series of changes is manifested by all members of the same race; it is a *type* of change characterizing many individuals. If now we ask why such a shifting of parts takes place we can investigate the racial forerunners of the organism in question. From comparative anatomical and palaeontological data there may be good reason for supposing that the condition in the animal or plant in question has been reached through an evolutionary shifting of the parts. In this way we should reach a phylogenetic explanation: a historical explanation in the stricter sense and one which could not be generalized because it would describe a unique series of changes characterizing an evolutionary succession.

When we turn from genetical explanations, whether ontogenetic or phylogenetic, to physiological or 'causal' explanations what is the difference? In the first place the physiologist (in the widest sense) is not at all concerned with unique occurrences but with types of change which can be generalized. This difference would not distinguish him from the descriptive embryologist who also deals with types of change which can be generalized. What distinguishes the physiologist's procedure is the fact that he does not simply record the changes normally observable, but the changes which are observed when the organism or its environment is systematically interfered with. He is thus able to discover more than the descriptionist: he is able to investigate the mutual interdependence of the parts, and the rôle they fulfil in pervasive types of change, i.e. types of change which can be generalized.

On the other hand physiology tells us nothing about the history of organisms. It takes an organism for granted as a going concern and proceeds to analyse it experimentally. Phylogeny can only tell us about history—about morphological changes. It appeals to genetic continuity to account for the resemblances between organisms and can only appeal to hypothetical intrinsic changes to account for their differences. There are, in consequence of this, certain dangers in the historical method which are sometimes overlooked. It tends to emphasize the resemblances and to slur over the differences. The latter are in danger of being neglected and treated as 'unimportant' or 'superficial'. The notion of evolution appeals very strongly to our monistic desire to 'reduce' everything to some one thing. But we are now beginning to discover that after an origin has been assigned to an organism there is still a great deal left out of account, and consequently, now that the first flush of enthusiasm is over, the historical point of view is not attracting so much attention.

Dr. C. D. Broad has pointed out two errors into which a too exclusive attention to the genetic method may lead us, and these are of interest to the biologist. He writes:

> 'We are extremely likely to underestimate the complexity and ignore the peculiarities of the final stage, because we cannot see how they could have been developed out of the earlier and simpler stages . . . it is much more disastrous to slur over differences which are really irreducible than to recognize differences and wrongly think them to be irreducible. If we make the latter error we still have in hand all the data for the solution of our

> problem, and we or others will solve it when we have pushed our analysis a little further. But, if we make the former mistake, our data are incomplete and the problem cannot possibly be solved until we have recognized this fact.'

The second type of error is closely related to the above:

> 'When I study the evolution of anything, be it an animal or an institution or a mental process, I am simply learning about the history of it and its "ancestors" in a wide sense of that word. I learn that A developed into B, B into C, and C into the thing in question. Now we are all extremely liable to confuse a history of the becoming of a thing with an analysis of the thing it has become. Because C arose out of B, and B out of A, people are inclined to think that C is nothing but A in a disguised form. . . . To analyse anything you must examine and reflect upon it; and the most elaborate account of what preceded it in the course of history is no substitute for this. At the best a study of the history of a thing may make you look for factors in the thing which you might otherwise have missed . . . you have no right whatever to say that the end is just the beginning in disguise. If, on inspecting the end as carefully and fully as you can, you do not detect the characteristics of the beginning in it and do detect the characteristics which were not present in the beginning.'[1]

The present tendencies in biology are a clear illustration of a gradual recognition of the truth of what Dr. Broad says, and it would be easy to find abundant instances of the consequences of the errors he mentions in the many speculations which have been supposed to 'follow' from evolutionary notions. The notion that 'C is nothing but A in a disguised form' results from the preformationist view of evolution—a view which, as we shall see, amounts to a denial of any genuine evolution, just as the old preformationist view of individual development amounted to a denial of real development.

I propose now to examine the nature of historical propositions in the strict sense, i.e. historical propositions as the historian knows them, and to contrast them with so-called historical propositions in biological science.

When a historian asks: Was William I. crowned? he means did the history which was William contain a slice knowable as the kind of occurrence we call 'coronation'? The evidence upon which he bases his answer consists for the most part of pieces of parchment with certain black marks upon them. These are interpreted as having been made by human beings either during the life of William I. or at some remote epoch near that time, and also as having been made to convey a

[1] *The Mind and its Place in Nature*, London, 1925, p. xx.

meaning. In other words the origin of such pieces of parchment is interpreted in accordance with what we know about the origin of such things at the present day. From such data, interpreted in accordance with such assumptions, the historian comes to conclusions, more or less probable, regarding the question whether William I. was crowned or not.

Now when a palaeontologist asks: Did the Permian reptiles leave descendants which in successive generations were transformed into mammals? he takes for granted that the fossils found in certain rocks are remains of extinct animals. Just as the historian takes for granted (with due precautions against fraud) that his manuscripts are literary remains from a remoter epoch. The palaeontologist's data for answering the question consist, then, of fossils interpreted in the above light which, on geological grounds, are regarded as having been deposited at successive periods. Finding a gradual change in these remains in a certain direction he interprets this as a change in descendants of some of the forms occurring at earlier periods.

Thus in both cases we have a *description* of past events—unique serial changes which are only supposed to have occurred once. But in both cases we can ask further questions. Regarding the coronation we can ask: Why? How? Where and When? And these further questions can only be answered, if at all, by a further examination of data of the same general nature as those upon which the inferences regarding the occurrence of the event itself depend. Similarly we can ask regarding the transformation of certain descendents of Permian reptiles: Why? How? Where and When?—and these questions too can only be answered, if at all, *in an historical sense*, by an appeal to data of the same general nature as those upon which the inferences regarding the occurrence of the event itself depend. The question Why? in the case of the palaeontological problem means, presumably: How did it come about that some reptiles were transformed, while others remained reptiles? Whereas the question How? means: What were the successive events in and through which the transformation was brought about?

Now it is evident that in attempting to answer the question How? a palaeontologist would certainly appeal to the same kind of data as before. He would hope to answer it, if it could be answered, by collecting a more complete series of fossils and

by subjecting them to a closer scrutiny. But it is also clear that the same procedure could hardly be expected to answer the question Why? in the above sense. To ask such a question is to ask what is commonly called the *modus operandi* of evolution. It is to demand a 'causal' explanation of the process. But as soon as we raise such a question we are no longer pursuing history, and we can no longer appeal to historical data. The data we now use are no longer comparable to the historian's manuscripts or the palaeontologist's fossils, but are all taken from processes going on *now* in organisms living at the present day. We are therefore using data of a type to which no historian would appeal. No historian would sit down and speculate about why and how William I. came to be crowned simply from his knowledge of the habits of kings at the present day—the thing would be absurd. But this seems to be what we are doing in biology when we talk about 'causal' explanations of evolution. The only data a historian would admit would be actual documents written at or near the time. Inferences from the present would be quite useless for dealing with the particular historical event, although they enter in a general way in leading him to interpret his documents as remains left by human beings. But it would be quite ridiculous to argue that because nowadays kings are crowned in Westminster Abbey by an archbishop and because William I. was a king, therefore William was crowned in Westminster Abbey by an archbishop. Moreover, to explain the 'How' of the coronation in terms of the specific gravity of gold and the neuro-muscular apparatus of a hypothetical prelate would be more ridiculous still—even although there is every reason to believe that the physical properties of gold and the physiological properties of muscle and nerve were the same in those days as they are now. The first inference would be ridiculous because we know that we have no grounds for inferring that because in modern times kings are crowned under the above conditions they have always been so crowned. The second inference would be ridiculous for a different reason. There is no reason to suppose that such an account of the matter would be incorrect, it might be blameless physiology, but it would not be *history*. All we have done is to extend certain inductive generalizations backwards in time to a particular case at a remote period. We assume that the laws of nature were the same then as they are now and we

infer that what happens now when muscles contract also happened then. But this does not give us genuine historical data about the precise details of the *actual* historical occurrence. It can only do that *if* we have genuine historical data obtained by the historical method to go upon. For example, *if* we know the precise weight of the crown and the height to which it was lifted it may be possible to calculate how much carbon-dioxide was evolved in lifting it on to the king's head, but without these historical data we can make no *historical* use of the inductive laws. History deals with the particular features of unique occurrences as such, and consequently genuine historical data alone are of primary value. Inductive generalizations deal only with pervasive general processes which are exemplified in many events and consequently are of no direct use to the historian. We can say that Caesar did not get his feet wet in crossing the Rubicon *if* he wore gum-boots and *if* the river was not too deep, etc., from our knowledge of the properties of water and gum-boots at the present day *if* those properties were the same at the time Caesar crossed the Rubicon as they are now. But only historical data would enable us to say whether it was probable that he wore gum-boots or that the water was not too deep.

Now we have seen that evolution is a unique event in the sense already explained.[1] Any assertions we may make about it will, therefore, either be genuine historical propositions based upon historical data, or, they will be propositions involving the extension backwards in time of inductive generalizations based upon observations of processes going on at the present day. The latter will therefore always be of the form: *If* such or such *was* the case (and this will depend upon historical data) then such and such will also probably have been the case, *provided* (1) the inductive generalization involved is well founded; and (2) the laws of nature were the same then as they are now. But if this is the case how can we speak of a causal explanation of evolution? We only discover causal regularities by putting things in certain relations and observing what happens, and then repeating a similar series of changes with similar things but with certain constituents omitted. But if evolution represents a unique series of unrepeated changes which is past how is that possible? We can only extend causal laws at present operating backwards in time

[1] See p. 393 above.

on the assumption that they have never changed, but if we do that will this not amount to saying that in evolution nothing has changed? In other words may not evolution be an evolution *in the laws of nature*, at least so far as organisms are concerned? This will require careful consideration later. Meanwhile there are other points about extending inductive generalizations backwards to be considered.

In natural science we extend inductive generalizations either backwards or forwards in respect to time, and, as I have already said, we assume in both cases that the laws of nature were or will be the same during the period either in the future or in the past into which they are extended. Now an important difference in the two cases lies in the fact that a prediction can be *verified* simply by the method of "wait and see" but a projection backward cannot be verified in the same sense and thus this important scientific demand cannot be fulfilled. A prediction professes to say what will take place. But, as we have seen, a backward extension of an induction cannot do the same kind of thing. It cannot tell us what *actually* took place, it can only tell us *what will also* probably have taken place *if* a certain change did in fact take place. Thus in the long run we always come back to some requisite datum for which we can only use the historical method. Now let us see how this applies to a particular example. Let us take Darwin's account of how the Giraffe came to acquire his long neck, which is given in Chapter VII of *The Origin of Species*. It is assumed, of course, that the Giraffe has evolved from an ancestor having a neck of more average length—either on actual palaeontological data, or on comparative anatomical data coupled with the general theory of evolution, since there is no reason to make an exception in favour of Giraffes. Darwin says:

> "That the individuals of the same species often differ slightly in the relative lengths of all their parts may be seen in many works of natural history, in which careful measurements are given."[1]

He then goes on to point out that although these differences are of no importance to most species

> ". . . it will have been otherwise with the nascent giraffe, considering its probable habits of life; for those individuals which had some one part of several parts of their bodies rather more elongated than usual, would generally have survived. These

[1] *Op. cit.*, edit. 1900, p. 277.

> and left offspring, either inheriting the
> s, or with a tendency to vary again in the
> . whilst the individuals, less favoured in the same
> respects, will have been most liable to perish.'
> ' . . . the individuals which were the highest browsers and were able during dearths to reach even an inch or two above the others, will often have been preserved; for they will have roamed over the whole country in search of food.'

Now it is at once apparent that the amount of historical data here available is extremely small. Even if we suppose we have a most perfect series of fossils showing a succession of forms with progressively longer and longer necks, this will not yield us the kind of data we require in order to make an estimate of the probability of the explanation offered. *That* changes in the immanent endowment of the race of nascent giraffes took place is guaranteed to us by the fossil series and the general theory of evolution which is not in question. But what we do lack are genuine *historical* data which will justify us in supposing that there were periods when all the food had been eaten up except that above a certain height so that those animals in this district which were below a certain tallness perished and those above it—even in the matter of an inch or two—survived; quite apart from the difficulty that the young of the tall ones would also perish during a dearth. *If* such a state of affairs were ever realized then *of course* only those over a certain height would have survived but did it actually happen? *Only* historical data could give an answer to this question and from the nature of the case such data are naturally not forthcoming. But in that case no causal explanation of how giraffes evolved is possible and it is just the lack of the requisite historical data which makes so much speculation about questions of this kind futile from the scientific standpoint. The whole story has a suspicious appearance of begging the question at all the critical points, and is the type of explanation which simply puts into the beginning just what is required at the end. You begin by saying that slight variations in length of parts are of no importance to most species and then proceed to assume that they were important in the one that you want to turn into a giraffe. You also assume that such variation in length of neck went on occurring in the same direction in later generations, and that the competition for food was so close that only the tallest survived, although this did not affect the young

ones. This is certainly not history, and as an inductive hypothesis it is quite untestable when applied to a particular case. The same remarks apply to the rival hypotheses. We are in possession of no inductive generalization regarding the *modus operandi* of evolution of such high probability and generality as will justify us in asserting with any confidence what happened in an historical example.[1]

We can now sum up the results of this section. Historical explanations describe the successive changes witnessed in developmental processes either racial or individual. But they do not tell us anything about the mutual relations of dependence of the parts in the latter case, nor about the relations of dependence of evolving organisms to one another or to their environment in the former case. These two cases are further distinguished by the fact that in the developing embryo we witness a series of changes which also characterizes a number of other events whereas the evolution of species is in the strict sense a unique series.

'Causal' or inductive propositions, on the other hand, do not deal with unique events but with pervasive types of change. Embryological propositions—even when 'merely descriptive'—are inductive and therefore not strictly historical. They differ from physiological propositions in being confined to the normal course of events, and thus state nothing about the mutual dependence of the parts. If inductions are extended into the future they can be verified, but they can only be employed scientifically in respect to past events if we have historical data to furnish the requisite minor premise, and if we are justified in assuming that conditions were such at the period in question that the inductive generalization would then have been valid.

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Preformation and Epigenesis

We saw in the embryological section how the notion of immanent genetic factors arose. If two rabbits in the same litter differ from one another—one, say, being black and the

[1] For detailed discussions of the logic of evolutionary and morphological theories see Tschulok's *Descendenzlehre*, Jena, 1922; and A. Meyer's *Logik der Morphologie*, Berlin, 1926.

other white—we seem driven to assume that a difference was in some way immanent in the ova from which the rabbits began, provided black and white have both been manifested during their ancestry, and provided, of course, that the difference is not traceable to environmental differences. Genetic factors do not 'develop'—they merely persist and are involved in the characterization of what develops or of what issues from the developmental process. I tried to reconcile preformation and epigenesis by making a distinction—not without difficulty—between the process of genuine increase of organization which was epigenetic, and the characterization of the parts thus developed, the latter alone being the concern of the genetic factors. But it had to be admitted that the *mode* of development of the *whole* (e.g. cleavage pattern) was also a character and would therefore be dependent upon the immanent endowment.

How, then, does the situation stand when we come to evolution or racial development? Since the evolutionary change is accomplished in a succession of individual organisms which successively depart from the typical characterization of the race we seem driven to assume a progressive change in the immanent factors. The theories of orthogenesis and natural selection both appeal to immanent changes. And the same is true of theories of the Lamarckian type, which differ from the former only regarding the way in which such changes may have come about. Thus any theory of evolution is driven to suppose change in the immanent factors. Orthogenesis apparently supposes that such changes occur in an orderly fashion in accordance with an immanent 'law', since such changes could not be correlated with general changes in the environment running parallel with the changes in the organisms. Natural selection supposes that they occur 'accidently', i.e. it makes no assertions about the origin of the changes, and hence it is not strictly speaking a theory about the *origin* of species, but only about their survival.[1] Lamarckian doctrines are the only ones which venture to offer a suggestion about the origin of immanent changes. They suppose that such changes occur in correlation with the mode of life of the individual organisms. When we remember how completely ignorant we are about immanent factors—since they are mere

[1] This fact has been obscured by the association of the selection hypothesis with the title of Darwin's book.

methodological postulates in the first instance to enable us to deal causally with the occurrence of different characters in closely related organisms—it is not surprising that we should be completely in the dark in regard to the question of how changes in them are possible. It should be borne in mind that this question of the possibility of change in the immanent factors is quite independent of theories of evolution. From the last section it can be seen that such theories can never be verified since we are dealing with historical problems for the solution of which the requisite data are not available. But the question of the origin of changes in the immanent factors is quite a different one. It is a process which may be, and in the case of mutations apparently is, going on to-day and is therefore one which is open to scientific investigation.[1]

Now we saw in the embryological section that the chromosome hypothesis identifies the genetic factors with particles in the chromosomes. I wish now to show some of the difficulties which make their appearance when we attempt to bring this theory into line with the doctrine of evolution, and also with another hypothesis—the hypothesis, namely, of the origin of living things from the inorganic constituents of the world. Speculations of the latter kind are usually carried out in complete disregard of many facts about organisms as we know them to-day, and it is desirable to examine them from the logical point of view to see what assumptions are involved. Such speculations obviously not only require *change* in the immanent factors but profess to give an account of their *origin*. It will not be difficult to show that speculations of this kind are highly unscientific and are only admitted into science because they do not conflict with the demands of traditional scientific materialism, although they do seem to conflict with what we are able to discover about organisms. Their affinities are much closer to speculative metaphysics and they should not be confused with scientific theories. They are most commonly indulged in by physiologists or biochemists who are apt completely to ignore the rest of biological science—a procedure which is perfectly legitimate in the laboratory, but which inevitably leads to contradictions in any attempt at a constructive theory.

[1] A thoroughly disinterested and scientific discussion of the present status of theories regarding species transformation see Mr. G. C. Robson's *The Species Problem*, London, 1928.

All speculations about the origin of life which conform to the type I am referring to begin with unstable chemical compounds and try to convert them into stable living organisms. As an example we can take the following remarks from Starling's *Principles of Human Physiology*. We must first note what is said about organisms. On p. 4 the author writes:

> 'At a certain stage in its life every organism divides, and a part or parts of its substance are thrown off to form new individuals, each of them endowed with the same properties as the parent organism, and destined to grow until they are indistinguishable from the organism whence they were derived.'

They are thus to have 'immanent factors' which are as like as two peas—'indistinguishable'. But of course it is not necessary to wait until the organisms grow up to discover this. Provided you compare the appropriate temporal part or slice of the histories they are indistinguishable all through—indeed more so (if that is possible) at the beginning than at the end. On p. 5 it is written:

> 'A living organism may be regarded as a highly unstable chemical system which tends to increase itself continuously under the average of the conditions to which it is subject, but undergoes disintegration as a result of any variation from this average. It is evident that the sole condition for the survival of the organism is that any such act of disintegration shall result in so modifying the relation of the system to the environment that it is once more restored to the average in which assimilation can be resumed.'

Thus the organism is represented by the physiologist as trembling on the peak of a metabolic curve like a celluloid ball on a jet of water in a gipsy shooting gallery. In order to overcome the dangers of its position it is endowed with a peculiar property which is explained in more detail in the following passage:

> 'Every phase of activity in a living being must be not only a necessary sequence of some antecedent change in its environment, but must be so *adapted* to this change as to tend to its neutralisation, and so to the survival of the organism. This is what is meant by adaptation.'

We have, then, an unstable chemical system tending to increase itself under average conditions. But whenever a departure from such an average occurs it *necessarily* undergoes disintegration, and such disintegration *must* also, according to the above passage, neutralize the environmental change which

has called it forth—if the organism is to survive. But this cannot be what is meant. The change in the organism does not neutralize the change in the environment—it 'neutralizes' the consequences *for the organism* of such changes. If the temperature of my room goes up the changes which take place in me do not 'neutralize' the change in my room but are such that *my* temperature does *not* go up. It seems then that our unstable chemical system requires two distinct parts: (1) a part in which changes are supposed *necessarily* to take place whenever the environmental conditions depart from the average, and (2) a part in which changes occur (whether 'necessarily' or not is not stated) which have the result of neutralizing the changes in part (1). Also the changes in part (2) are disintegrations, upon the proper working of which the survival of the organism depends, whereas the changes in part (1) are presumably assimilations. It seems that every organism requires these two parts as an irreducible minimum before it can be *called* an organism and not a 'mere' chemical system. Chemical systems as we find them in nature do not have these two parts although they survive, hence their method of survival must be of a different nature.

Now we are given an account of how the first type of chemical system evolved out of the second. During the 'chaotic chemical interchanges' which are supposed to have occurred when the earth was cooling down we have to imagine that

> 'some compound was formed, probably with absorption of heat, endowed with the property of continuous polymerisation and growth at the expense of surrounding material.'

One of the worst features of speculations of this kind is that we have to *endow* our entities with just the properties we want them to have, and this smacks too much of 'finding bad reasons *for* what we believe upon instinct'. It is difficult to see how from a logical point of view this procedure is any more respectable than that of the vitalists. The story continues as follows:

> 'Out of the many such compounds which might have come into being, only such would survive in which the process of exothermic disintegration tended towards a condition of greater stability, so that the process might come to an end, and the organism or compound be enabled to await the more favourable conditions necessary for the continuance of its growth.'

In this passage the author is in some doubt about whether

to call the thing he has conjured up an organism or a compound, but judging by the criterion already given it was undoubtedly a compound because parts (1) and (2) have not yet been elaborated, otherwise it would not have to 'wait' for more favourable conditions. It clearly persists by an inorganic method. Not instability but stability is here postulated so that the compound may call a halt in the disintegrative process when conditions become too stormy. But now the author borrows from biology, not from chemistry:

> '. . . and in all probability the beginning of life as we know it was the formation of some complex substance analogous to the present chlorophyll corpuscles, with the power of absorbing the newly penetrating sun's rays and utilising them for the endothermic formation of further unstable compounds.'

Now it will be noticed that so far all that has been done is to take parts of organisms as we know them and to suppose that they could exist as parts independently. Unstable organic compounds and chlorophyll corpuscles do not persist or come into existence in nature on their own account at the present day, and consequently it is necessary to *postulate* that 'conditions' were once such that this did happen although and in spite of the fact that our knowledge of nature does not give us any warrant for making such a supposition. It is not history since from the nature of the case we can have no historical data. Neither is it induction because such assumptions fly in the face of all our present inductive knowledge. It is simple dogmatism—asserting that what you want to believe did in fact happen. Starling gives no account of how parts (1) and (2) came to be established but falls back again on his *unstable* compound and, given that, the rest—apparently—is simple:

> 'Once given an unstable system, such as we have imagined, the great principle laid down by Darwin, viz., survival of the fittest, will suffice to account for the production from it by evolution of the ever-increasing variety of living beings which have appeared in the later history of this globe.'

But before this great principle will work we require (1) organisms which vary in all directions so that some are 'fitter' than others, and (2) reproduction through which other organisms come into existence with the same favourable characters, and (3) some form of competition. In other words we require entities which are capable of spatial repetition by division

yielding two or more organisms with the same mode of organization, i.e. a persistent 'immanent endowment'. Moreover to satisfy the author's own demands regarding 'adaptation' the organism has to be equipped with parts (1) and (2) as already explained. Thus an 'explanation' of this kind can only make out a case for itself by begging the fundamental questions at issue—the essential characteristics of an organism have to be surreptitiously introduced in vague general language. It is a good example of the type called 'describing the unknown in terms of the known'—by leaving out those parts of the known which it is convenient to omit and introducing, by the convenient method of 'postulating,' just those which are necessary to make the explanation appear plausible. Now clearly this circular type of explanation is in no sense scientific and can neither be tested nor entertained as a working hypothesis. Theoretical biology has been ruined in the past by speculations of this kind. Such 'explanations' have no place in serious scientific literature and would be unheard of in physics. It seems therefore that Driesch is perfectly right in saying:

> 'The question about the so-called primary origin of life is as incapable of being discussed as is the problem of death, in spite of the great number of popular works written about it.'[1]

17

Let us leave these speculations about a hypothetical beginning and start again at the other end of the evolutionary story. Consider a newt and a frog developing in the same pond. The differences between them, which become more and more manifest as their development proceeds, is an expression, we suppose, of differences between the immanent endowments of the two races to which they belong. Now according to the doctrine of descent these two races will ultimately converge to one race—i.e. to the common ancestors of newts and frogs. This race will therefore either possess the immanent factors proper to both newts and frogs or we have to suppose that in some of the immediate descendants of these common ancestors a change of a certain type occurred in the immanent factors, and in others a change of another type—and that this type of change continued in the same direction in each case in at least

[1] *Science and Philosophy of the Organism*, London, 1908, p. 262.

some of the further descendants. That is to say in some the change must have gone on occurring in a froggy direction in at least some individuals, and in others the change must have gone on occurring in a newty direction in at least some individuals. For the present let us assume the first alternative, namely, that the common ancestor had a richer immanent endowment than either its froggy or its newty descendants. If we adhere consistently to this supposition (as some biologists are inclined to do) we see that in general as we recede from the present into the past the race-events which are converging towards one another become richer in immanent factors, until, when we reach the primitive amoeba-like mother of us all, we reach a maximum of richness of immanent endowment. But we have amoebae still with us. Are they then as rich in immanent endowment as their predecessors, or have they partly lost it? If they are still as rich how have they contrived to conceal the fact? If they have lost part of their original endowment why have they not also changed? Professor T. H. Morgan has an argument touching on this point which appears to me to be a very bad one. On p. 309 of *The Theory of the Gene* he says:

> 'If the same number of genes is present in a white blood corpuscle as in all the other cells of the body that constitutes a mammal, and if the former makes only an amoeba-like cell and the rest collectively a man, it scarcely seems necessary to postulate fewer genes for an amoeba or for a man.'

What this argument wants to assert seems to be this: Let x be the number of genes in a human white blood corpuscle, and let n be the number of cells in the human body. Then if x genes 'make only an amoeba-like cell' and if $nx - x$ genes make 'collectively a man' then 'it scarcely seems necessary to postulate' fewer genes for an amoeba than for a man. No indication is given regarding how we are to get from the amoeba-like cell in the premise to the amoeba in the 'conclusion'. These two entities resemble each other in certain abstract respects. They both exhibit the cell-type of organization, and they both exhibit certain metabolic changes which are common to all entities having that type of organization. But it would be hazardous in the highest degree to identify them on these grounds, since these are abstracted features common to almost all living things. One is a whole organism and the other is an organic part. It would be safer in the

present instance to compare a white blood-corpuscle with a human ovum than with an amoeba, for we know that the blood corpuscle has been derived by division from the ovum, and a human ovum is a very different kind of entity from an amoeba. But now does any one suppose that genes *make* cells or that they *make* men? We have seen that Professor Morgan himself states that the chromosome hypothesis 'does not profess to state how factors arise or how they influence the development of the embryo' (See above p. 362). And we have seen what great difficulties surround the whole question of the relation of genes both to the characters and to what is thereby characterized. How then can we speak of genes *making* cells? Cells never are *made*, they merely persist by division and elaboration. But the argument seems to say: *x* genes make a white blood corpuscle, therefore *x* genes can make an amoeba, and all you have to do is to multiply *x* genes by *n* and *nx* genes will make a man. And this seems to suggest that a man is a conglomeration of *n* amoebae, but whether this is really what is meant it is difficult to say. The essential point is this: genes are postulated to account for the persistent differences in the characterization of genetically related organisms. Now the difference between a man and an amoeba is not simply a numerical difference in the number of cells which constitute those two organisms. If the number of genes bears any relation to the number of 'characters' which distinguish one organism from another then, since the number of characters appears to be much greater in man than in an amoeba it would seem that there would be many more genes in the nucleus of a human ovum than in the nucleus of an amoeba, and if all 'somatic' mitotic divisions are equal there will be the same number in a human white-blood corpuscle as in the ovum. Genes are concerned with characters not with 'making' cells. But of course we know so little about them and about their numerical relations to characters that arguments of this kind seem useless. How many characters has a man? How many genes has he? These questions must be answered before we can discuss such arguments.

Returning now to our original question about whether the hypothetical ancestral amoeba had a richer endowment of immanent factors than its descendants we see at once what difficulties such a supposition lands us into when we are

asked to go further back still with Starling. Amoeba, like all living things, divides and thus hands on complete sets of immanent factors to new spatially separated individuals. But if we go further back we are supposed to reach a stage where reproduction by division does not take place but living things were created by the sticking together of inorganic bodies. What, then, happened to the immanent factors? According to some of the supporters of the gene theory they are themselves single organic molecules. We have already seen reason to doubt the truth of this hypothesis. Now we seem driven to believe that the first organisms had immanent factors (genes—organic molecules) which were more richly endowed than any of those of their descendants. Thus according to the backward way of reading the story the first organisms, although only 'unstable chemical systems' should have the maximum immanent endowment. Would any chemist, examining chemical compounds that can be synthesized to-day, have any inkling that he had to ascribe such properties to them?

Thus as long as we read the story backwards all goes well until we reach the hypothetical beginning. Then the whole scheme seems to be absurd, unless we are prepared to say that atoms and molecules possess 'immanent factors', or at least that the first synthesis had a richer endowment than any later developments therefrom. Then we have to suppose that by repeated division (accompanied, of course, by anabolic synthesis) these immanent factors were gradually sorted out to produce the successive 'forms of life'. This account of the matter represents evolution as a process analogous to the distintegration of the radio-active elements in which a simple unpacking is supposed to take place. But whereas in the chemical example the whole thing is supposed to be gradually unpacked, in the biological example only a *part* of the organism is unpacked, namely the nuclear chromatin, and with a very curious result. As the unpacking proceeds the organisms in successive generations exhibit *greater* elaborateness of organization and characterization although the immanent factors upon which the latter is supposed to depend become *fewer*. These, then, are the difficulties we get into when we start with the assumptions of modern genetical theories and read the story backwards. We find an original abiogenesis very hard to believe.

But suppose we begin with an original abiogenesis and read the story in the forward direction. We then have to suppose, as we have already seen, that inorganic entities did what they do not appear to do now: namely they proceeded to combine together to produce progressively more and more elaborately organized bodies. In order to do this we have to suppose that 'conditions' were very different then from what they are now—it being tacitly *assumed* that such inorganic entities existed then with the same intrinsic properties as they have now. Now when once we have to appeal to hypothetical conditions at remote times, different from those we now know, we can say little or nothing about such times, because all our laws of nature are dependant for their validity on the continued operation of conditions *now* operating, and we can say absolutely nothing about the empirical laws of a possible nature under totally different conditions. In other words we are no longer arguing from the present to the past but are simply asking that the past should have been such as to have made possible what we are trying to explain, viz. the origin of living things. Hence we are merely proceeding in a circle.

Nevertheless, suppose we waive all this and grant that the process has been set going. These unstable compounds now proceed, as we have learnt, to 'struggle for existence'. These sons of the Adamless Eve proceed to butcher one another after the manner of Cain and Abel, or at least to compete for the available means of continuing their polymerization. But the 'struggle for existence' is a highly 'teleological' notion which ill asserts with unstable chemical compounds. Do unstable organic compounds struggle for existence at the present day? They either have to be preserved in sealed bully-beef tins, or they become a prey to 'higher organisms'. But in those days there were *ex hypothesi* no higher organisms and hence no bully-beef tins. But if these compounds could be formed they would also, presumably, be in no need of protection. None the less we are told that they were unstable and that was why they fell to struggling for existence. But does not this savour too much of wanting to have the best of both worlds? Because organic compounds are unstable under present conditions we suppose that they were unstable under the hypothetical past conditions. But they are not formed now under 'natural' conditions: how then can we suppose that they were formed and yet were unstable under the hypothetical conditions?

But even if we let this difficulty pass others still confront us. Even if we skip lightly over all such difficulties on the nimble wings of faith we are still met at every step with the problem of how increase in elaborateness of organization could come about. Somewhere we must stop making drafts on hypothetical 'conditions' under cover of which miracles may happen. We are required to reach a stage where modern genetical theories become practical politics, and this means that our organisms were equipped with immanent genetical factors upon which their characterization depends, and which were capable of progressive change and multiplication by division. Natural selection will not help us at all because it only deals with the survival of immanent changes, not with their origin. It helps us to see how some attempts towards new modes of organization would not be successful after they had been reached, but not how they were reached.

Thus the question of increase in organization is the crux of the whole problem. Reading the story backwards it did not trouble us. Starting with the modern genetical theories we started with a given level already achieved, and as we passed back in the converging series we simply handed on this achievement to more and more remote ancestors in the form of representative particles. But when we got back to the beginning this procedure seems to land us in absurdities or contradictions. That is the consequence of the 'preformationist' view which refuses to admit a genuine development. It supposes that the immanent factors were gradually lost and the immanent endowment as a whole became less complex, and this, as I have said, has the further curious consequence that as this process continued the organisms exhibited *more* manifest complexity of characterization, so that with fewer immanent factors we have more characters. Now it is desirable to recall at this stage that the only entities we know and observe to-day to increase their mode of organization are those having the cell-type of organization, and this only occurs in metazoa. In protozoa persistence is achieved through whole-producing division. But in metazoa persistence is achieved through part-producing division. In other words, in the latter case the property of spatial repeatability of the cell-type of organization is, so to speak, 'made use of' in *individual* persistence with a consequent increase in level of organization. But in the changes studied by genetics we do not witness

increase in organization but variation in the mode of characterization.

18

It is now high time to leave these speculations in which evolution is chiefly thought of as a process of increase in structural complexity, in order to consider it a little from the physiological aspect. Professor Pembrey, writing of the effects of the doctrine of evolution on physiology, says that 'it is a matter for surprise that they are not more obvious and definite'.[1] He expresses the opinion that this is because 'in recent times physiology has been studied and even defined as the physics and chemistry of life'. I have no doubt that this has had a great deal to do with it because the notion of evolution is quite foreign to the traditional concepts of physics and chemistry. But there are other reasons. In the first place evolution has largely meant, in the minds of biologists, the study of phylogenies, and this has been a purely spatial science—three-dimensional solid geometry—a series of forms being strung on a time series. Phylogeny has been history—the history of a series of form changes. Physiology, as we have seen, has nothing to do with history, i.e. with unique changes, but with the kind of repeatable pervasive types of change which are called causal regularities. Other factors which may also have something to do with the failure of physiologists to take much interest in evolution are (1) the widespread belief among them that the doctrine of natural selection explains everything about evolution and makes it scientifically respectable; (2) erroneous notions about 'adaptation'; and (3) the difficulty of taking evolution seriously in conjunction with the classical scientific materialism.

Now if, as has been urged, the structural aspect of organisms is a highly abstract one in which time is not taken seriously but is regarded as something external, it should not surprise us if a view of evolution which is largely founded on such a basis should prove to be very one-sided. If it is simply a question of structural complexity with 'fitness' at different levels, it is difficult to attach any meaning to the terms 'higher' and 'lower' as applied to organisms. A man and the tapeworm he harbours in his gut are both fit in their

[1] In *Evolution in the Light of Modern Knowledge*. London, 1919, p. 183.

respective environments, and differ only in their structural complexity. If we consider the evolutionary transition from a fish to an amphibian it is easy enough to correlate this in a general way with their respective surroundings—water in the one case and air in the other. There is no question of 'higher' or 'lower'—one is fit in the one medium and the other is fit in the other. But when we compare a reptile with a mammal, or the whole evolutionary 'adaptive radiation' of reptiles with that of mammals, this interpretation does not seem to suffice. And yet we speak of mammals as being higher than reptiles, and we regard a fish as higher than a medusa. From a purely morphological point of view the differences are differences of structure. But there is no virtue in mere structural complexity as such which would justify the use of the term 'higher'. The meaning of the terms higher and lower only becomes apparent when we leave the morphological point of view and regard the organism as an organized system of events among a complex of environmental events which are not organized as parts of one enduring thing. A mammal is higher than a reptile because it is *independent of its environment* to a greater degree than a reptile—a familiar instance of which is seen in the existence of heat-regulating processes in mammals. The reptile behaves in relation to the external temperature just as any inorganic body does, but in a mammal this is not the case, since, within certain limits, it preserves a constant temperature. Now when changes in an organism are directly correlated in a simple one-one relation with changes in the environment we can call such changes in the organism *inorganic reactions*. But when, on the other hand, changes in the environment are followed by changes in the organism which render it independent of such environmental changes, then we can call such changes in the organism *biological responses*. They evidently imply a level of organization above the inorganic level. Consequently an organism can be called 'higher' than another organism if in a given set of circumstances it exhibits biological responses and the other organism does not. Thus a medusa is carried by the tide just as any floating body might be, but a fish is able to swim against the current (always of course within certain limits) and preserve a given position. An evolutionary series may be called 'progressive' if the succeeding members show a gradual substitution of inorganic reactions by biological

responses. Consequently evolution along the progressive lines represents a process of achievement of increasing independence of contingent environmental circumstances. Where organisms are said to 'possess minds' rendering prevision possible it is not difficult to see[1] that an enormous possibility of independence of contingent circumstances will thereby be achieved. This is well illustrated by the view of those who hold that the chief aim of science is prediction in the service of man.[2] Progressive evolution thus appears to be a process of evolution towards freedom from environmental contingency. Accordingly the notion of 'adaptation' does not express adequately what seems to have occurred in progressive evolution. Two organisms may be perfectly adapted to their environments and may be successful in the struggle for existence, and therefore, from the point of view of adaptiveness and survival value they may be on an equal footing, and neither is more or less 'high' than another. But one may have a greater degree of freedom than another. Every one knows that closeness of adaptation may prove to be a disadvantage to a race of organisms if it means loss of plasticity of response. Adaptedness is a clumsy method of persistence because it relies on persistences in the environment. There is no such thing as adaptation if by that you mean a *state*, because a living organism never is in a state since temporal differentiation belongs to its very essence. But there are relatively stable or pervasive features in the environment and what we call adaptations of structure are the visible appearance of fixity of organization in correlation to such pervasive environmental features. Such 'stagnations' in the organization and mode of characterization of the complex of events which is known as the living organism, and habit formation in the psychological sphere, are examples of adaptedness with loss of plasticity. If we could believe that such stagnations arose in the first place as biological responses which became incorporated in the rhythm of the race and persisted in virtue of their success we might find a reconciliation between Darwinism and Lamarckism. But such speculations must, it seems, ever remain beyond the reach of empirical verification and will therefore perhaps always belong only to the realm of faith.

[1] i.e. from the 'intuitive' if not from the speculative standpoint.

[2] Although they frequently deny this possibility from the speculative standpoint.

Thus among organisms we seem to have the following modes of persistence: (1) adaptedness—relying on persistences in the environment. In a given organism some parts will always be of this nature—e.g. respiration, stream lines, etc.; (2) plasticity of response[1]—e.g. regeneration, substitution of biological responses for inorganic reactions, intelligence (if we admit psychological factors); (3) spatial repetition by division in the biological sense with persistence of immanent endowment.

Evolution, then, so far at least as the progressive lines are concerned, cannot be regarded as fundamentally a process of increasing complexity of anatomical structure—that is only an abstract aspect of it. Here as elsewhere we must think of the organism in terms of characterized events. The physiological aspect is accordingly the more concrete although it can only be studied by comparison of organisms now living. But from this point of view evolution along the progressive lines appears to have been a process of achievement of increasing freedom from contingent environmental circumstances by various modes.

Now in one of the three senses of the term function there has been no evolution of function. All living things share certain fundamental 'metabolic activities'—assimilation, respiration, and so forth. And this fact seems to have been partly responsible for lack of interest in evolutionary ways of thinking among physiologists. But these fundamental 'functions' are only the physiological aspects of organized systems of events which are known to us in visual perception as having the cell-type of organization. And just as, in developmental processes, we witness visually a gradual increase in levels of organization achieved in virtue of the spatial repeatability of organic parts having this type of organization, in just the same way these fundamental 'functions' are built up into systems in which the individual activities are subordinated as part-events organized in a particular way which is able to issue in biological responses. Thus although it is true in an abstract sense that the "fundamental vital activities" have "remained the same", they have entered as part-events into modes of elaboration which have evolved.

[1] See below, pp. 436-8.

19

With the results of the preceding sections in mind we now have to brace up our wits for the extremely difficult task of a final assault on the problem of preformation and epigenesis. We have seen how deeply rooted is the objection to admitting the possibility of development as a real process. This objection was operative all through the XVII. and XVIII. centuries against individual development—so powerfully indeed that people actually saw under the microscope the human body preformed in the sperm. This difficulty is overcome, in part at least, when we find it traceable to a fundamental property of the cell-type of organization. But the same objection is still operative, as we have seen, against the recognition of racial development. We must concentrate, therefore, on the question of the choice between admitting and denying increase in complexity of the immanent endowment of a race. We saw that if we assume decrease of immanent complexity with temporal passage we come into conflict with the doctrine of an original abiogenesis. If decrease in immanent endowment with evolution is true then either (1) the original abiogenesis was a miracle, in the sense of a process for which our present knowledge can give us no hope of understanding, or (2) it did not happen, i.e. the original 'complexity' came to this earth from elsewhere, which simply drives the miracle away to another part of the universe. If we do not like either of these alternatives we seem to be driven to admit progressive increase in complexity of the immanent factors in the course of evolution. We should then have increase in immanent complexity with increase in manifest elaboration, instead of the paradoxical increase in the latter with *decrease* in the former. The consequences and nature of this admission must therefore be considered. We shall not then conflict with the doctrine of an original abiogenesis nor with modern genetical theories provided the latter are couched in purely conceptual terms; but is such a view compatible with genetical doctrines expressed in terms of material particles after the manner of the classical materialistic tradition? Before we proceed further in this last and most difficult stage of our study of this antithesis it will be well to collect together what we seem entitled or required by empirical data to say about immanent factors:

(1) They are capable of persisting through enormous periods of time with very little change, being correlated with the more or less rhythmical repetition of the characterization of those events which constitute the life of each individual in a given race—and with extraordinary precision, even in the face of considerable environmental variation, and to a large extent independently of what happens to the individual in consequence of contingent environmental circumstances.

(2) On the other hand, if evolution is true these immanent factors have, along certain lines, undergone a progressive *change* in a definite direction correlated with the substitution in the individual of inorganic reactions by biological responses issuing in new modes of persistence in virtue of increasing independence of contingent environmental circumstances.

(3) But there does not appear to be correlated with this in any very clear way a gradual increase in volume of the germ-cells or their nuclei. But measurements bearing on this question are obviously very difficult to make and to interpret.

(4) They cannot be 'known as' chemical molecules because they are required to be capable of repeated division without loss of specificity. But they are required to have an atomic character, and a definite persistent organization *inter se*.

(5) They are hypothetical entities *postulated* in accordance with the assumption that differences between natural entities must either be traceable to outside events or have existed prior to the manifestation of the difference in some 'occult' form. (See footnote on p. 335).

Now the difficulty of reconciling these requirements—especially the second—with immanent factors conceived as 'little hard lumps' is sufficiently obvious, and we might interpret Bateson's celebrated British Association Address as a refutation of such a view. We have been trying to conceive evolution with the aid of concepts borrowed from a sphere which knows nothing of such a process, namely the classical mechanics of the XVII. and XVIII. centuries. Now it is an important maxim in science not to try to explain the wrong things. Goethe, in one of his letters, wrote: "The greatest art in theoretical and practical life consists in changing the problem into a postulate; that way one succeeds". It may be, then, that in trying to explain evolution with the aid of the notion of the material particle borrowed from

mechanics we have gone the wrong way to work. Prof Whitehead, writing of physical endurance, says:

> '. . . physical endurance is the process of continuously inheriting a certain identity of character transmitted throughout a historical route of events. . . . This is the exact property of material. . . . Only if you take *material* to be fundamental, this property of endurance is an arbitrary fact at the base of the order of nature; but if you take *organism* to be fundamental, this property is the result of evolution.'[1]

In every explanation something is taken as fundamental and the rest is interpreted in terms of this. If you take the material particle as fundamental you are confronted with the problem of *how* evolution is possible. Wherever we have a set of characters persisting through a period of time so that we find the same set of characters in every temporal slice of that period we have a persistent material thing, which offers little aid in interpreting evolution and is itself, according to Prof. Whitehead, an arbitrary fact at the base of the order of nature. But if you turn your problem into a postulate taking evolution as fundamental, as a process in which modes of persistence are progressively achieved, then the material persisting thing is itself interpreted as an outcome of evolution and is no longer an arbitrary fact.[2] Moreover, the cell-level of organization is itself the outcome of evolution, representing a new mode of persistence above that of inorganic organized bodies, because when it was reached it proved to be capable not only of being 'transmitted throughout a *historical* route of events', but also, as I have pointed out, of *spatial* repetition. And when we come to the metazoa we find this property making further progressive change possible, since this type of organization can be repeated to yield *parts* in one organism, yielding possibilities of infinite variety in modes of elaboration for the building up of biological responses and hence new modes of persistence. Thus what develops in the individual is its organization, what develops in the race is its mode of persistence.

It has often been said that one of the great merits of the Darwinian theory rests on the fact that it explains evolution without appealing to any but 'natural causes'. There is

[1] *Science and the Modern World*, p. 135. See footnote, p. 339 above.

[2] That is to say evolution may be a 'category'—a principle of explanation, not a 'problem' to be explained in terms of other categories.

no such thing as a 'causal explanation' of evolution. The fundamental requisite—immanent change—is simply taken for granted, and the doctrine is committed to ascribe to 'bits of matter' properties which they do not exhibit to-day, as we have already seen. People who say these things do not appear to have thought at all deeply about evolution, but would seem to be in precisely the same position as the religious thinkers to whom Prof. Whitehead refers in the following passage, where, speaking of the rise of evolutionary ideas during the XIX. century, he writes:

> 'By a blindness which is almost judicial as being a penalty affixed to hasty, superficial thinking, many religious thinkers opposed the new doctrine; although in truth, a thoroughgoing evolutionary philosophy is inconsistent with materialism. The aboriginal stuff, or material, from which a materialistic philosophy starts is incapable of evolution. This material is in itself the ultimate substance. Evolution, on the materialistic theory, is reduced to the rôle of being another word for the description of the changes of the external relations between portions of matter. There is nothing to evolve, because one set of external relations is as good as any other set of external relations. There can merely be change, purposeless and unprogressive. But the whole point of the modern doctrine is the evolution of the complex organisms from antecedent states of less complex organisms. The doctrine thus cries aloud for a conception of organism as fundamental for nature. It also requires an underlying activity—a substantial activity—expressing itself in individual embodiments, and evolving in achievements of organism.'[1]

But orthodox biology, far from searching for an adequate conception of organism, has been trying to interpret organisms by taking bits of matter as fundamental on account of the success of that notion in physics. But we are now learning how criticism from three points of view—epistemological, physical, and biological—is converging to displace that notion from the fundamental position it has hitherto held in the philosophy of nature. Thus biology is being forced in spite of itself to become biological!

We must remember, however, that we are here confronted with a choice not of alternative interpretations within a given metaphysics of nature, but we are invited to revise that metaphysics itself. Turning our problem into a 'postulate' in this way simply means a change in our attitude

[1] *Science and the Modern World*, p. 134. Cf. J. S. Haldane: 'Science must ultimately aim at gradually interpreting the physical world of matter and energy in terms of the biological concept of organism.' *Mechanism Life and Personality*, p. 99.

to what we are to regard as fundamental. How the details are to be worked out is another matter. And in this place we are not concerned with the extension of the notions of organism and evolution in the way contemplated by Prof. Whitehead, but with the particular problems of biology itself. What in detail would be the consequences of such a change of front remains to be seen, but one consequence is sufficiently obvious. It would help to jog biology out of the rut of orthodoxy into which it has sunk in the belief that XIX. century physics provided a basis of unquestionable metaphysical certainty which rendered any criticisms of foundations unnecessary. It would encourage the development of independent biological thinking, and an unprejudiced return to the consideration of observed facts and what they suggest. But of course it would not in the least prejudice biophysical and biochemical investigation as contrasted with speculation.

How, then, do the difficulties we have been confronted with in this chapter appear if we abandon the notion of material particles (as ordinarily understood) as a vehicle for the interpretation of genetical data? And I raise this question from the standpoint of critical theory not from that of heuristic convenience. The important fact that has emerged is that such simple minded notions lead us into contradictions and apparent absurdities when we try to think them out in evolutionary terms. If we regard what is persistent in a given race as represented by particles which undergo periodic reshuffling and are handed on from one individual to another we are driven to admit either that a complete set was present from the start or that sets were progressively built up by additions from without. We also are asked to conceive these particles as causes related to the manifest persistent characters as effects, and this as we have seen (p. 364) seems to require constancy, not only in these particles but also in the medium in which they are supposed to 'work'. This also requires constancy in the relations between cells since a given character involves mutual relations between masses of cells and their relation to the whole—'transcendence of cells'. In other words, the constancy of characterization of the whole for a given 'normal' environment, which is what we observe and begin from, seems to be required even for the carrying through of the hypothesis that this characterization is causally dependent upon the constancy of only a

part, namely the hypothetical particles. This then seems to be the position into which we are led by the attempt to bring genetical data into relation with embryological and evolutionary ones, and clearly these difficulties rest on the fact that our ordinary modes of thinking do not provide for dealing with developmental problems. It is the old difficulty of reconciling persistence, change, and temporal passage. What persists in an organic race is the mode of characterization for a given environment. But this mode of characterization is not of the kind which can be displayed 'at an instant', because an organism is such that with temporal passage new *spatial* parts come into being and hence its characterization changes. But for a given period of its history it will have a mode of characterization according to the race of which it is a part, and this can be matched with corresponding periods in the histories of its predecessors and successors. What passes and is therefore temporal is the 'given period'; what persists[1] is the mode of characterization of that period. The hypothetical particles are invented to bridge the gap between a given period of one history and the corresponding period of the next. What we observe bridging the gap is the rest of that history and the first part of the next, which latter begins as a separated part of the first exhibiting the cell-type of organization. This type of organization is itself persistent, i.e. a character. It is the entity of which this is the persistent character which is spatially repeated in development. But the developmental process cannot adequately be described in terms of such entities and their parts but requires reference to their organizing relations which change as the new spatial parts come into being. Thus characters do not develop, they persist throughout a race although only characterizing certain temporal parts of the race, and consequently the genetical theories are difficult to bring into relation with the theories of development. Developmental theories, whether individual or racial, do not deal with characters but with processes, i.e. with organisms as events in relation to the events constituting their environments. Accordingly we are brought to the conclusion that this puzzle of preformation and epigenesis is but an aspect of the much wider question of the relation between the events of nature which pass and the persistences of nature, and between

[1] in the race.

what is actual and what is possible and how possibilities become actual. The scientific way of dealing with such problems is to regard the possibilities as 'stored up' in what is now actual. The future possible explosion is thought of as stored up in the gun powder in the form of 'potential energy', and the future characters of the embryo and adult are thought of as stored up in the germ cell in the form of particles causally related to them. We think in this way because this is the way of thinking which is suggested to minds trained in dealing with problems on the common-sense level of experience. Many people are inclined to follow M. Bergson in concluding that scientific thought can only work in this way and that consequently there can be no science of the organism. But a more hopeful alternative would be to try to overcome these infirmities and develop other ways of thinking. For if our minds could develop one way of thinking they could also presumably develop others, but if we adhere fatalistically to the one way we shall never find any other.[1]

Dr. Julius Schultz has written an able and subtle discussion on this antithesis.[2] He is a follower of Vaihinger and divides all scientific theories into hypotheses and fictions. The former are distinguished by the fact that they admit of empirical testing, whereas the latter, although they may be valuable for purposes of generalization, are for ever removed from empirical verification. He traces the antithesis between preformation and epigenesis to an unavoidable antinomy from which spring two possible views giving rise to two different types of fiction which can never be reconciled because they can never be put to the empirical test. Dr. Schultz's first contention is that the fundamental biological problem is a form problem—all biological phenomena resolve themselves he says into processes in which specific form is either brought into existence or preserved. This I should not assent to because form is only an abstracted aspect of the organism resulting from the separation of space and

[1] Cf. W. James: 'It cannot be too often repeated that the triumphant application of any one of our ideal systems of rational relations to the real world justifies our hopes that other systems may be found also applicable. . . . Nature may be remodelled, nay, certainly will be remodelled, far beyond the point at present reached. Just how far?—is a question which only the whole future history of Science and Philosophy can answer.' *Principles of Psychology*, II, 671.

[2] *Die Grundlehnungen der Biologie*, Berlin, 1920.

time. But it is evident that for Dr. Schultz even 'function', following the scientific tradition, also reduces to 'form' since he obviously thinks of it as consisting of a change in the spatial relations of particles.[1] Dr. Schultz goes on to explain that in order to make assertions about organic form we require a general concept to express the persistence of form in the midst of change of stuff or metabolism. We require a correlate to the category of substance as employed in physical science, i.e. to the concept of matter which expresses the persistence of stuff with change of form. Dr. Schultz gives the name 'individuality' to the required concept. But experience fails to conform to the fulfilment of this postulate since we find unceasing change of form. 'Thus from the union of the categories of individuality and change a category of lower level is born: the concept of "development".'[2] In this way (according to Dr. Schultz) arises the notion of an individual as that which develops its form 'out of itself according to its own rules' in contrast to dead things whose form merely persists until modified by outer influences. From now onwards according to Dr. Schultz further thinking out of the situation leads to an antinomy since it is evident that the outer world must in some way contribute to the development. But how are we to conceive it? Are we to treat the external events as merely 'conditions' for a developmental process which is internally determined, or does the environment work to the goal together with the 'internal constellation' on 'equal terms'? On p. 22 Dr. Schultz says:

> 'If the outer world is to determine true development, as the epigeneticist will have it, it cannot work mechanically on the germ, it must rather work through a supersensuous medium whose creative forces it excites into activity: the analogue to which is our own behaviour excited by impressions from without. Experience can never decide between this view and that of evolutionism which supposes all possibilities of future form already preformed in the germ.''

Thus Dr. Schultz seems to suppose that epigenesis means that development starts from a structureless beginning and that the only alternative is to suppose 'all possibilities of future

[1] Cf. *op. cit.*, p. 14, where he says: 'Es wandelt also Einflüsse von einem Körperteil her die Funktion eines anderen beständig ab; die Funktion: das bedeutet aber doch wohl die Struktur im kleinsten Raume.'

[2] *Ibid.*, p. 20.

form already preformed in the germ'. But from what has been said it will be seen, I think, that this is a false way of stating the antithesis. Dr. Schultz's arguments rest on foundations and a priori modes of reasoning from which I have been trying to escape, and it is not necessary to go over all this again. But the last sentence in the above passage is interesting in connection with the conclusion to which we have reached, namely that this antithesis is a 'special case' of the much wider philosophical problem of the relation between possibility and actuality. And connected with this is the use of the causal postulate. Only what is manifested is actual, and we think of what we expect will be manifested as 'intrinsic properties' or 'possibilities'. This works well enough for individual development because we begin with an entity having the cell-type of organization with certain assumed intrinsic properties. These are manifested through an epigenetic process, which is epigenetic because parts not previously present gradually come into being in virtue of the spatial repeatability of that from which we start, and because, with this, new organizing relations come into being. Hence the intracellular elaborations which appear in these parts can be interpreted as resulting from their intrinsic properties and their organizing relations. There is nothing here to contradict the causal postulate. But when we come to evolution we are required to suppose that these intrinsic properties gradually become *enhanced*. The entities having the cell-type of organization acquire *new* intrinsic properties, which they did *not* possess before, if evolution is epigenetic. Moreover, not merely characters depending on intracellular elaboration are involved, but new modes of organization, in virtue of which greater independence of environmental contingencies is possible. Thus new modes of elaboration of new *parts* are involved, and this presupposes new 'intrinsic properties' and it is just the possibility of this which is in question. In individual development we assume what intrinsic properties we want in accordance with the causal postulate. But to assume this in the case of evolution is either to assume that it is a purely 'preformationist' process, which conflicts with what we seem to observe and also with the doctrine of an original abiogenesis, or it is to beg the very question at issue. Hence the 'categorial' nature of such a process as evolution. Either the cell-type of organization,

or rather the entities exhibiting it, are capable of enhancing their intrinsic properties 'of themselves', or the changes in the environment are capable of enhancing them. But how did the environment enhance the intrinsic properties of the cells of the iris of the newt's eye to enable them to restore the lens when it is picked out—an operation which is hardly likely to have occurred in nature without destroying the whole eye if not the whole head? We are left with the denial of evolution as an epigenetic process as the only alternative—and thus with 'evolutio' in the old sense, as a final position. It is interesting to recall what Driesch has said on this point in connexion with the 'concept of univocal determination' as he calls it:

> 'The concept of the *univocal determination* of being and becoming may be called the very starting point of a philosophy of nature. No states and no events in nature are without a sufficient reason for their being such as they are at such a place and time, and the same thing always is or happens under the same conditions. These are the most general expressions of the principle of uncourse nothing in the doing of entelechy is opposed

On the next page he writes:

> 'Our principle of necessity or univocal determination relates to *everything* that may be or happen in the universe, without any reference to the character and nature of the changes in the case of things that happen. . . . It would be quite inconceivable to assume anything else, though our assumption leads to the consequence—strange as it is—that nothing really new can happen anywhere in the universe. *All happening is "evolutio" in the deepest meaning of the word.*'

One wonders how any one can be on such intimate terms with 'the universe' at large as to be able to make such comprehensive assertions about it as these. But if epigenesis is 'inconceivable' there is no other recourse but to deny abiogenesis. Dr. Schultz appreciates this point, and accordingly his fictitious 'biogens' are eternal and indestructable. Perhaps, then, the deepest question which can be raised on behalf of biological knowledge is this: How can the 'possibilities' of a natural entity be enhanced? And the only answer seems to be: By evolution. In other words, if evolution is epigenetic nature cannot be wholly 'uniform'.

[1] *Science and Philosophy of the Organism*, 1908, p. 153.

In considering these difficulties raised by evolution one is reminded of what Prof. Whitehead has said of time: 'It is impossible to meditate on time and the mystery of the creative passage of nature without an overwhelming emotion at the limitations of the human intelligence.'[1]

Before concluding this long chapter I would remind the reader that my primary aim has been to analyse the difficulties of this antithesis as clearly as I can rather than to offer an alternative to existing speculative theories. The topics of Division II of this chapter appear to me, for obvious reasons, to be of little importance for *scientific* biology, but those of Division I revolve round one of the most important themes for future investigation, namely, the problem of the relation between genetics and embryology. In regard to this problem there appears to be room for great improvement in our existing notions, as I have tried to show, but such suggestions as I have ventured to make in this direction do not profess to be more than tentative. The distinction between parts and characters appears to me to be important and fundamental. Also it is a mistake to regard characters as dependent simply on 'genetic' and 'environmental' factors, since the organism never simply consists of genetic factors. Accordingly I have used the term 'immanent factors' for *all* factors postulated on the side of the organism, and of these only *some* will be the factors postulated by Mendelian genetics.

[1] *Concept of Nature*, p. 73.

CHAPTER X

The Antithesis between Teleology and Causation

Although the notion of teleology is regarded as a thoroughly unscientific one, and is numbered among the bogies of biological thought, we are perpetually encountering modes of thought and expression in the works of biological writers of all creeds which are in some sense 'teleological', even although apology is sometimes made for them on the grounds of convenience of expression. This fact—and it certainly is a fact—calls for some careful examination. We require to clear up some of the confusion which attaches to the term teleology and we require to know in what sense it is involved in biology and how it is that teleological modes of expression are so difficult to avoid in biological description and discussion. I shall first give some quotations from eminent biological writers which illustrate the above tendency and also testify to the *heuristic* value of teleological notions.

The first is from Starling's *Principles of Human Physiology* and has already been given in part in the preceding chapter. On p. 5 he writes:

> 'Every phase of activity in a living being must be not only a necessary sequence of some antecedent change in its environment, but must be so *adapted* to this change as to tend to its neutralization, and so to the survival of the organism. This is what is meant by "adaptation". Not only does it involve the teleological conception that every normal activity must be for the good of the organism, but it must also apply to *all* the relations of living things. It must therefore be the guiding principle, not only in physiology . . . but also in the other branches of biology.'

Also on p. 8 of the same book this author says:

> 'The principle of adaptation is the only formula which will include all the phenomena of living beings, and it is difficult to see how this principle can be expressed by means of the concepts of the physicist.'

The next example is from Sir Charles Sherrington. Any one reading his celebrated book, *The Integrative Action of the Nervous System*, can hardly fail to be struck by the highly 'teleological' nature of the systems of reflexes described and by the importance of this aspect in their interpretation. On p. 235 the author himself calls attention to this when he says:

> 'The purpose of a reflex seems as legitimate and urgent an object for natural inquiry as the purpose of the colouring of an insect or blossom. And the importance to physiology is, that the reflex cannot be really intelligible to the physiologist until he knows its aim.'

and on p. 238 he adds:

> 'We cannot but feel that we do not obtain due profit from the study of any particular type-reflex unless we can discuss its immediate purpose as an adapted act.'[1]

Thus both of these physiologists testify to the heuristic value of the study of 'purpose' and in the context they both appeal to an *historical* explanation coupled with an inductive theory (natural selection) to account for how this purposiveness *came about*. The next example is also from a physiologist, but one who approaches the question from a different standpoint. Dr. J. S. Haldane, at the end of the first chapter of his *Respiration* (p. 14) writes:

> 'From a consideration of the general characteristics which distinguish a living organism from a machine I had become convinced that a living organism cannot be correctly studied piece by piece separately as the parts of a machine can be studied, the working of the whole machine being deduced synthetically from the separate study of each of the parts. A living organism is constantly showing itself to be a self-maintaining whole, and each part must therefore always be behaving as a part of such a self-maintaining whole. In the existing knowledge of the physiology of breathing this characteristic could not be clearly traced. The regulation of breathing did not, as represented in the existing theories, appear to be determined in accordance with the requirements of the body as a whole; and for this reason I doubted the correctness of these theories, and suspected that errors had arisen through the mistake of not studying the breathing as one of the co-ordinated activities of the whole body. In so far as the investigations detailed in succeeding chapters originated with me,

[1] It is interesting to contrast the above passages with the assertion of Prof. Marshall that 'there is nothing to be gained for biological research by seeking to interpret life in terms of ends or purposes'. See above pp. 214 and 248. For further testimony on the value of 'teleological' notions see Stolzmann: Arch. f. Entwicklungsmechanik. Bd. 108, p. 646, and Enriques, *Problems of Science*, p. 377.

> they were mainly inspired by the considerations just mentioned; and, as will be seen in the sequel, the same considerations have led to a reinvestigation and reinterpretation of other physiological activities besides breathing.'

Thus although Dr. Haldane does not use the terms 'purpose' or 'teleology' he speaks of 'requirements of the body as a whole' and of the 'co-ordinated activities of the whole body' and he is distinguished from most physiologists in having a concept of organism which leads him to *expect* certain features which are apparently *thrust upon* other physiologists. One more example is from a cytologist. Professor E. B. Wilson, writing about cleavage-form on p. 1005 of *The Cell*, says:

> 'All these considerations drive us to the view that the simpler mechanical factors, such as pressure, form and the like, are subordinate to more subtle and complex operations involved in the general development of the organism. This conclusion is strikingly illustrated by the phenomena of telolastic division. The constant succession of unequal divisions, always in the same plane, is here correlated with the apical growth of the embryo, and this expresses a deeply living law of growth in animals of this type. This is quite analogous to the definite forms of division in the apical cells of plants. In all such cases we cannot comprehend the specific forms of cleavage without reference to the end-result of the formative process; and the problems here encountered cannot be separated from those of development in the larger sense. The teleological aspect of cleavage thus suggested has been recognized more or less clearly by many observers; most adequately perhaps by Lillie,[1] who has urged that with this principle in mind "one can thus go over every detail of the cleavage, and knowing the fate of the cells, can explain all the irregularities and peculiarities displayed". The egg is not merely a cell dividing as best it may, under the stress of simple and obviously mechanical conditions. It is "a building which lays one stone here, another there, each of which is placed with reference to future development". Of the truth of this answer must, I think, be convinced who has critically studied these phenomena. Such a conclusion need involve no mystical doctrine of teleology or of final causes. It means only that the factors by which cleavage is determined are in greater or less degree bound up with an underlying organization of the egg that precedes cleavage and is responsible for the general morphogenetic process. The nature of this organization is almost unknown; but we can proceed with its investigation only on the mechanistic assumption that it involves some kind of material configuration in the substance of the egg.'

Now this statement of Prof. Wilson, like the first passage quoted from Sir Charles Sherrington, amounts to little more

[1] F. R. Lillie, *Embryology of the Unionidae*, Journ. of Morphology, Vol. X, 1895.

than saying that in regard to explanation a knowledge of the *outcome* of a biological process is often more illuminating than knowledge of what went before. We have seen that explanation usually involves bringing what is to be explained into relation with something else, and in biology the 'something else' is often the outcome of the occurrence to be explained rather than other events which preceded it. Thus the maturation divisions of the germ-cells would be quite 'unintelligible' if we knew nothing of fertilization.[1]

Having thus seen some examples, and heard some witnesses, bearing on the use of 'teleological' notions in biology, we must next examine some of the meanings of the term teleology in order to clear away misunderstandings and avoid ambiguity in what follows.

Actions are ordinarily said to be 'teleological' when they are directed to the realization of a purpose. The term 'purpose' should be confined to conscious human purpose. The purpose is some end or consummation that the man desires, whether it be to still the pangs of hunger, to attend a meeting of a learned society for the achievement of intellectual values, or a concert for the enjoyment of aesthetic ones. Prof. Hobhouse states that teleology involves three elements: (1) a process in time with some definite result; (2) an element of value in the result; and (3) it implies that this element is a determining factor in the process by which it is brought about.[2]

[1] An instance of the heuristic value of teleological notions is furnished even by physics. Prof. Whitehead writes: 'We find in Maupertuis a tinge of the theologic age which preceded his birth. He started with the idea that the whole path of a material particle between any limits of time must achieve some perfection worthy of the providence of God. . . . Maupertuis' success in this particular case shows that almost any idea which jogs you out of your current abstractions may be better than nothing. . . . Maupertuis had discovered the famous theorem of least action.' *Science and the Modern World*, p. 77.

[2] *The Theory of Knowledge*, p. 382. On the topics of this chapter the biological reader should consult Prof. Hobhouse's *Development and Purpose*, a book which deserves to be better known among biologists. For a detailed discussion of the modern position in regard to teleology see Ungerer, *Der Teleologie Kants und ihre Bedeutung für die Logik der Biologie*, Berlin, 1912. Other references are given in Sect. 4.

But the notion of purpose has been extended in three ways. In the first place it is asked whether the universe, or a mountain, or an animal is 'purposive'. Then, by analogy with human purposes, it may be asked whether such things are the purposes, or are related to the purposes, of some superhuman mind. Such questions obviously do not come within the compass of natural science, and it is the mixing up of such extensions of teleological notions as these with scientific questions, that has very naturally contributed to bring discredit upon the notion of teleology.

The second direction in which the notion of purpose has been extended is in regard to the processes going on in organisms. We say, for example, that the purpose of the heart is to keep the blood in motion. The word 'purpose' is obviously used here in a different sense from that given in the beginning of this section. It is not intended to imply that the heart consciously desires to keep the blood in motion, or even that the animal consciously desires to keep its blood in motion. But the use of the same term in these two senses tends to perpetuate confusions and obscures the issues involved in the use of teleological notions.

There is yet a third extension of the concept of purpose which helps still further to confuse the issues. Machines are spoken of as 'purposive' and we also speak of the purpose of a part of a machine in much the same sense as we speak of the purpose of a part of an organism. Machines are purposive in the sense that they are means to the realization of conscious human purposes. If a man wants to go from Oxford to London, various machines are available for the attainment of that end, and apart from such human purposes such machines would not have come into existence, and if such purposes ceased to be operative such machines would be so much useless lumber. These machines are only purposive in the sense that they are devised and put together by men for conscious human purposes. The machine is not a 'purpose' but merely an aid to the attainment of an end. But when, on the other hand, we speak of the purpose of a part of a machine we mean something quite different. We mean that the characters of the part are such that without it the machine would not work, and would therefore fail to achieve the human purpose for which it was made. And when we speak of the purpose of a part of an organism we mean

that this part exhibits some character which is such that without it the organism would not *persist*. Or, it may be that the character in question is dependent upon some other part, and in such a case the preservation of this character is called the purpose of the latter part. Thus certain chemical characters of the blood in vertebrates depend upon the kidneys, and its circulation through the tissues depends on the heart. The 'purpose' of these organs is to maintain these characters of the blood, and the 'purpose' of the blood is dependent upon the maintenance of these characters. But it is not necessary to use the term 'purpose' in this connexion, since it is simply an extension of one of the three meanings of the word 'function' as explained on p. 327, the further implication being attached to it that the persistence of the organism as a whole is also involved. Similarly when we speak of an act as 'purposive' we mean that through it the persistence of the organism or race is ensured. And such an act is spoken of as purposive even when no conscious purpose is supposed to be involved. Consciously purposive acts appear to constitute one type of a much wider kind of act. Even in human beings consciously purposive acts form only a small part of a great number of acts which have an equal title to be called purposive in the sense of having value for him. The beating of his heart, and the reflex closing of an eyelid are as purposeful as the building of a house or the putting up of an umbrella. And as far as organic parts are concerned a distinction between their 'purpose' and their 'purposiveness' seems rather artificial, and dependent upon a separation of 'function' and 'structure'. For the heart as it is in the living body is a beating heart, there is no separation between 'heart' and 'beating'.

After these preliminary remarks we can now return to the opinions of those biological writers which have already been given. It will be evident from these quotations that a number of different ideas are involved in 'teleology' as that term is used in connexion with organisms. In the above passages we find such terms as 'adaptation', 'survival', 'good', 'purpose', 'aim', 'end-result', 'self-maintaining whole', and 'requirements of the body as a whole'. How is it

possible to make these expressions precise, to determine what exactly they mean, and to bring the aspects of the organism to which they give expression into relation with others?

Little more need be said about Prof. Wilson's passage. It asserts no more than that at present we can interpret the cleavage processes better by beginning at the end than by starting at the beginning, but that there should be another way of interpreting it after the analogy of a 'wound-up clock' which is set going at the beginning of development and goes through its performances in virtue of its material configuration. This of course is 'static teleology' of the old fashioned sort, resting on a separation of space and time.

Starling's statement is not very helpful because if it is taken quite literally it would mean that organisms were immortal and incapable of 'making a mistake'. If *every* change in an organism is a *necessary*[1] consequence of an environmental change which preceded it, and if every such change *must* be for the 'good' of the organism (the word *must* occurs five times in this passage) then it is difficult to see what room there is left for 'accidents'. And accidents *do* happen even to the best adapted organisms. We have to remember the 'ateleological' occurrences, as well as the 'teleological' ones.

When Sir Charles Sherrington says that we cannot understand a reflex until we know its purpose, and that the purpose is a legitimate object of natural inquiry, all that seems to be meant is that you have to consider how the persistence of the organism is dependent upon the characters of a given reflex. And this seems to be such an obvious and innocuous proposal that it is astonishing to find a physiologist considering it necessary to apologise for it, and calling attention to its legitimacy and desirability. But tradition is difficult to overcome, and the word purpose rings harshly in the ears of a physiologist. It is very largely a matter of inventing a palatable terminology. Dr. Haldane is more explicit, and his terms are different, but I doubt whether what he contends for amounts at bottom to more than is maintained by Sherrington. A good deal depends upon what we are to understand by the expression "determined in accordance with the requirements of the body as a whole". Dr. Haldane

[1] See next section, p. 443, below.

says that it is not sufficient to study the parts of an organism separately, nor even to put them together to see how each one depends upon others. Each part is related in some way to all the other parts in such a way that the persistence of the whole is dependent upon this relation. It is this feature which is absent in the machine. A machine is *made* to realize some conscious human purpose. Its parts work together to secure *that* purpose, not to secure its own persistence. An organism is a mode of persistence, and its mode of persistence is different from the inorganic mode manifested in a machine. Machines are subordinate to organic persistence; they are used by human organisms for the purpose of securing their own mode of persistence, or the persistence of something that may be valuable to them. A machine may in fact be regarded as a part of an organism of a peculiar kind, linked to the rest by psychological as well as biological ties. This is sometimes expressed by saying that in the case of machines we have to do with 'external teleology', and in organisms with 'internal teleology'. Dr. Haldane's contention, then, seems to be that the physiologist requires to study that relation of a given part to the whole of the other parts in virtue of which the organism persists. But, as we saw when discussing organization, the relations of parts in an organism are internal relations, and this does not appear to be the case with machines. Consequently, this is another reason, and a very important reason, why it is not sufficient, as Dr. Haldane points out, to study organisms piece by piece as the parts of a machine can be studied. And it may prove to be the case that it is the existence of such internal relations which is responsible for the peculiarities of 'internal teleology'.

Some of the authors quoted above used the term 'adaptation' or 'adapted'. This term cannot be defined without reference to entities of which the terms 'living' and 'dead' can be significantly predicated. In physics and chemistry we are not usually concerned with the *persistence* of one thing, and consequently the notion of 'adaptation' does not arise. This term—like 'heredity', is one of those abstract substantives which are so liable to lead us astray. It will be advisible to stick to adjectives. Things are said to be adapted to one another. Organisms are adapted to their environments. And all that this means is that their characterization is such that in those environments they are

able to live. But it is possible to be more analytical than this. It is possible to analyse the organism into parts and show that these are adapted to corresponding environmental parts, or to parts into which the environment is analysable. We can express all these features of the organism in the following way. In an organism a given part-event is *significant* of something other than itself. The characterization of a given part depends upon that of others, and other parts depend in the same way upon it. All this is embraced by the concept of organization. But not only are the parts mutually dependent upon one another, but each is related to the endurance of the whole in the following way. The whole organism is a complex of organised events amid the complex of events constituting its environment, and its endurance is the outcome of the mutual relation of its parts to one another and to these environmental events.

Now in nature the environmental events are divisible according to their characterization into the persistent, pervasive, regular, or recurrent, on the one hand, and the irregular or contingent, on the other. When we say that an organic part-event is adapted to some environmental event we usually mean that it has some enduring character in correlation to some enduring character of the environment, and that in consequence of this the organism as a whole endures. Thus we find organic part-events exhibiting unchanging or rhythmical characters in correlation with regularities in the environment. Breathing, for example, is rhythmical in correlation to the constant character of the respiratory medium, and in grazing ungulates, not to mention plants themselves, the intake of food almost takes on a similar character. The characters of bones and all the more 'stagnant' parts of the organization exhibit obvious correlation with enduring features of the environment. Similarly, rhythmical regularities in the environment, such as seasonal changes, have their correlates in shedding and growth of hair, etc. The same is true of reflexes, 'instincts', habits, and indeed 'mechanisms' of all kinds. These all presuppose enduring environmental features of which they are *reflections* in the organism. They can all be studied in terms of causal laws. And they are all instances of 'adaptedness'.

But what are we to say of the *contingent* and irregular environmental occurrences? Here we obviously require

variability of character in the organic part-events. If those parts are to be in an organic relation to the environmental events and to the whole, i.e. a relation such that the endurance of the whole is thereby secured. If we are to speak of 'adaptation' here it obviously *cannot* be a 'state'. These environmental contingencies are of very different kinds. Some may be without significance for the organism. Some—like volcanic eruptions and storms—may overwhelm it. But to some it is able to make 'biological responses'. And among these may be contingencies which neither the organism nor its race has previously encountered. There is, however one kind of environmental contingency in relation to which we can discover a 'mechanism'. The environment includes other organisms, and the mode of occurrence of such organisms may be contingent. Of special importance are food organisms and mates. Accordingly we find a wide spread method among animals and plants for overcoming the contingency of the occurrence of such environmental organisms, namely, locomotion. And this 'device' is 'employed' (to use 'teleological' language) in two principal ways: either we find locomotion of *parts* produced in large numbers (e.g. spermatozoa and pollen) or locomotion of the *whole*, coupled with the elaboration of part-events which are *selective* in their relation to environmental events, e.g. receptors. Thus the receptor-effector system, which is analysable into reflexes interpretable as 'reflections' of enduring environmental (or intra-organic) characters, is a 'device' for 'dealing with' certain types of environmental contingency, and the whole essence of which is *variability* of response. But as soon as you introduce variability *within* the organism in this way you upset the placid regularity of those part-events which are 'adapted' to the enduring environmental features. Accordingly the respiratory, excretory, and circulatory part-events which are rhythmical or continuous, have also to be variable, and so are brought indirectly into relation with the environmental contingencies. But although variability is thus brought inside the organism the relations between these part-events is not contingent and their variability is under the 'control' of a mechanism. The contingency is without, and the primary variability of response lies between the organism as a whole and the environmental contingencies. Thus we have to distinguish between adaptedness as something

accomplished and 'provided for' by a mechanism of some kind; and the *process* of *perpetual* change in response to environmental contingencies, which cannot possibly be regarded as a 'state'. It is the latter feature of the organism—a new mark of 'thinghood' as it was called in Chapter III—which is so difficult to understand with our present ways of thinking. Any change in an organism following an environmental change either subserves the persistence of the organism or it does not. If the former is the case we can call it an 'appropriate' response. Now with our present ways of thinking there appear to be two ways of regarding such an occurrence. We can either ask: Did this happen 'appropriately' *in order that* the persistence of the organism should ensue? Or, we can ask: Did this just happen in this way, and it 'happened' to be appropriate? The difference is expressed in the ordinary use of the terms 'deliberate' and 'accidental'. If we say that the 'appropriate' response occurred *in order that* the organism might persist, we are giving a teleological answer in the strict sense of the term. This is what vitalism does. But if we say that the change just happened and was accidentally appropriate, this is equivalent to saying that *no* answer *can* be given. And it may be that this is all that it will ever be possible to say. It may be that the question is *meaningless*, because human thought (perhaps) cannot deal with particular occurrences. This will be more evident after the next section. Meanwhile it is important not to confuse such a case with a case of 'adaptedness'. Wherever there is a reflex or a habit, or any other sort of mechanism which is repeated we can investigate the state of affairs with a view to an explanation in causal terms. No difficulty is presented regarding the 'appropriateness' of such cases unless we ask about their origin. All we can do if this question is raised is to say that such a thing happened once as an 'accident' and because it was appropriate it survived. But this, of course, must not be confused with a 'causal explanation' as is sometimes done. All biological explanations of the 'wound up clock' type are confronted with the difficulty of how the winding up took place in the first instance. And then we have to suppose either that the clock has been wound up for all eternity, or that certain races acquired (by accident) the property of producing such clocks. Thus here, as in the former case,

we seem to be driven into an extra-scientific region where we can only erect untestable hypotheses. All such appeals to the past (unless they have regard only to historical matters of fact for which fossil evidence may be obtainable) involve untestable hypotheses.

The problems which cluster round the 'teleological' aspect of organisms seem to require that it should be conceived as a kind of growing point in the course of temporal passage. This growing point carries with it from the past unchanging features which subserve endurance of the organism because they are in correlation with persistent environmental features. The visible and tangible aspects of these 'stagnant' part-events make up what we call 'structure'. But temporal differentiation is essential to organic endurance, it is an 'intrinsic' character. Moreover, an organism persists by 'responding', hence there must be perpetual change in relation to relevant environmental contingencies. Hence part-events require to be 'fluid' as well as 'stagnant'. The new temporal parts which are added on to the duration of the organism with each pulse of time, are, so to speak, slices of perpetually *changing* adaptedness. They have to conform to what has already been realized on the side of the organism (including what is called its 'previous experience') and also to the contingently characterized environmental events now being realized. One would suppose that it was here at this growing point that we should have to look for the origin of adaptedness, but the difficulties with our present ways of thinking are obvious and enormous. Human thought, like some of the adaptednesses themselves, deals with the universal and regular, rather than with the particular and contingent. The above paragraph is a relapse into mysticism.[1] Whether complete silence would not be preferable, or whether there is some other alternative seems doubtful.

To sum up: the difficulty of avoiding 'teleological' modes of expression in biology rests partly upon the fact that in an organism the parts constituting it are so related to one another and to environmental events that typically the organism endures. An organism is therefore capable of biological responses, as well as inorganic reactions. Evolution along progressive lines requires a gradual elaboration of biological responses issuing in greater and greater independence

[1] See footnote on p. 156.

of environmental contingencies. In so far as a mechanism for such responses has already been elaborated they are susceptible of a causal analysis. But from such a point of view neither *particular* appropriate acts of response which do not belong to a routine in relation to an environmental routine, nor the *first appearance* of those embodied now in a routine, can be treated at all. They are accidents. Only two types of theoretical biology have so far been devised, both involving using the analogy of a humanly constructed machine: (1) vitalism (with a mechanic), and (2) the 'machine theory' (without a mechanic). This provides no independent *biological* way of thinking, because machines presuppose organisms. The vitalist puts himself into the machine he has made. The other type of theorist forgets he has made it. You obviously cannot *escape* from teleology in this way, because machines are teleological instruments made by men. Any explanation of teleology by analogy with machines simply attempts to explain internal teleology by means of external teleology and hence still remains teleology. The machine theory without a mechanic illustrates the way in which biology has turned either to one or the other of the two modes of thinking resulting from the Cartesian dualism. It only seems 'materialistic' because the psychological origin of the machine is easily forgotten and omitted from the analogy. Any one who wishes to spare himself further thought on this troublesome question can do so by adopting either of the current alternatives, but he will not thereby avoid 'teleology'.

We turn now to causation. And here I have little to add to what has already been said in Chapter III. At the present day the whole subject of causation is in a chaotic state. Men of science, on the one hand, who use the notion, appear to be too busy amassing facts to trouble themselves much about what they mean by it, and philosophers, on the other hand, seem, for the most part, from the time of Hume onward, to entertain beliefs on this subject which are very difficult to harmonize with the use of the notion in biological science. Consequently the following review of the situation seems hardly an exaggeration:

> 'In the whole history of philosophy, confused as it is, there is scarcely any subject in such utter confusion as causation. There are references to it in the writings of his predecessors, but Hume was the first writer of note who discussed it at length, and he got it into a tangle which has been worse and worse entangled by subsequent writers, until the latest contributors to the discussion have essayed to cut the knots by denying altogether that there is such a thing as causation.'[1]

This being the state of affairs it is somewhat unprofitable and premature to discuss an *antithesis* between 'teleology' and 'causation' when both notions are still so lacking in clarity, and their use is not yet freed from the fog of ignorance, dogmatism, or unacknowledged assumptions. A proper study of this antithesis requires far more attention than I have been able to give to it, and all I can profess to do here is to call the attention of the biological reader to some of the diverse issues involved, and to urge the desirability of devoting more deliberate thought to it in its relation to biological problems. Whereas teleology is regarded as a thoroughly unscientific notion, causation is commonly believed among biologists to be entirely respectable from the scientific standpoint. Biologists in general do not seem to be aware of the difficulties which cluster round the latter notion, and it is not therefore surprising to find it being used uncritically and leading to untenable positions. Of this we saw an instance in the last chapter. And yet, in spite of all the criticisms that have been levelled against these notions it is difficult to see how we can get on in biology without using *both* of them in *some* sense. The difficulty, as I have said, is to make them precise, and to free them from 'subjective' and 'anthropomorphic' elements.

A great many different but related problems and ideas are involved in the use of the notion of causation. We start with the broad fact of *change*. Change is apt to be contrasted with 'permanence' or 'persistence', but in the organism both notions are involved, for we see that continual intrinsic change is essential to its persistence, and in Chapter III it was urged that causal regularities are themselves 'permanences in nature' in the sense there explained. Many people appear to suppose that 'persisting unchanged' does not require

[1] C. Mercier, *On Causation*, London, 1916, p. 1. This book is interesting as a vigorous protest against current philosophical views on causation from the somewhat restricted standpoint of outraged common sense. It hardly seems correct to say that Hume was the first writer of note who discussed causation 'at length'.

to be explained, and that 'causes' explain changes. But this seems to be a mistake resting on an illegitimate use of 'explaining'. We cannot explain persistence or change as such. When we speak of explaining a given change we simply mean referring it to a type of causal regularity which is known to have characterized previous durations containing elements of the same type as those in question in the particular case. Thus, as Dr. Broad says:

> 'If a bishop falls down in the street it is antecedently more probable that this is due to a piece of orange peel than to direct diabolical agency. For although both hypotheses explain the observed fact equally well, the former fits in much better with the other facts which we know about the world than the latter does'

Coupled with this belief that causes have to be invoked to explain change is the belief that 'causes' *force* or *compel* changes to happen. But this seems to be based only on certain types of causal processes, particularly those of the pushing and pulling type involving human behaviour. It is therefore usually considered that the idea of the cause compelling the effect is an importation into causal sequences of feelings accompanying such human actions.

A great deal of the dispute about causation in the past has centred round the use of the notions of 'cause' and 'effect'. The upshot of these discussions seems to be that it is impossible to make these notions precise, and that, however useful they may be in daily life or even in scientific investigation, they are useless for theoretical purposes. As was explained in Chapter III these terms only give expression to certain outstanding features of what is a continuous process (as far as we can observe), and in which many elements are always involved. If we abandon these terms we simplify our problem to the extent of avoiding such perplexities and inconsistencies as we find, for example, in Mill's discussion of causation, and we also avoid the dangers which attach to a too naïve view which demands to know 'the cause' of evolution, or development, or 'cancer', etc.

Another notion involved in discussions of causation is 'determinism'. On this I will merely quote a passage from a recent critical physical writer:

> 'By determinism we understand the belief that the future of the whole universe, or of an isolated part of it, is determined in terms of a complete description of its present condition. . . . It

> is popularly assumed that every physicist subscribes to some such thesis as this. But now if there is infinite structure even in a small isolated part of the universe, a complete description of it is impossible, and the doctrine as stated must be abandoned. It seems to me that all present physical evidence prepares us to admit this possibility. I suppose, however, that most physicists would subscribe to some modification . . . perhaps along the following lines. Given a description of an isolated part of the physical universe in the most complete terms that have physical meaning, that is, down to the smallest elements of which our physical operations give cognizance, then the future history of the system is determined within a certain penumbra of uncertainty, this penumbra growing broader as we penetrate to finer details of the structure of the system or as time goes on, until eventually all but certain very general properties of the original system, such as its total energy, are for ever lost in the haze, and we have a system which was unpredictable.'[1]

The reader may be left to his own meditations on the implications of this passage for biology.

We have next to consider the causal postulate, causal laws and the causal nexus. The causal postulate directs us, as we have seen, where we find differences, to look also for other correlated differences. Its use presupposes causal regularities, i.e. pervasive or recurrent types of change. Consequently changes which do not belong to a type (if such there be) could not be dealt with by the causal postulate. In biology the demands of the causal postulate are frequently not fulfilled, if we adhere to what is perceived. And it is this failure which leads to the postulation of imperceptible changes. This is well illustrated by reference to ' mnemic phenomena '. If an organism behaves differently on different occasions to the same stimulus, it is necessary, either to take the organism's previous history into account, or to regard its history as ' represented ' in it, i.e. past changes have to be regarded as persisting ' stored up ' in it. The former is the alternative proposed by Mr. Bertrand Russell[2] in his theory of ' mnemic causation ', which is recommended on the ground that it does not involve appeal to imperceptibles; the latter alternative is the one which is always taken for granted by biologists. Thus Dendy, referring to the ' phenomena of memory ', says that these are ' reasonably explained ':

> ' by supposing that impressions received by certain cells of the brain may be stored up as " engrams " in these cells for use on

[1] P. W. Bridgman, *The Logic of Modern Physics*, p. 210.
[2] See his *Analysis of Mind*, London, 1921, Lecture IV, pp. 77-92.

> future occasions, when, under appropriate stimulation, they give rise to mental conditions corresponding to those produced by the original stimuli.'[1]

In general our ordinary notions of causation do not meet with any difficulty in dealing with organic changes of the destructive or katabolic type. It is with change of the constructive, progressive, developmental or adaptive type that the difficulty is felt. And the commonest method of dealing with such cases is to appeal to explanations of the 'wound up clock' type, in which what is to happen is already accomplished, and is only waiting to be 'released'. The difficulty which is felt in regard to such cases is perhaps partly dependent upon the fact that changes of the 'destructive' type are so much more familiar in our experience of the inorganic world. The two chief points about the use of the causal postulate, then, are first that it presupposes recurrent types of change and secondly that it leads us to deny 'spontaneity', or that differences can arise without previous correlated differences, or that anything happens that does not belong to a type.

Causal laws represent the embodiment in knowledge of the recurring types of change discovered under the guidance of the causal postulate. It is in connexion with these and their use in inductive inference that most of the criticisms of causation have arisen.[2] And the two chief points which have been disputed are the necessity and the universality of causal laws. It is not necessary to repeat the usual arguments here. They will be found in Venn's book,[3] and a more recent and more drastic criticism is that of Mr. Bertrand Russell in the chapter on the notion of cause in his *Mysticism and Logic*. The upshot of it all is that the old 'Law of Causation' as it is found in Mill and Bain, and which certainly is not justified by experience, is given up, and something more modest, called by Mr. Russell the 'principle of the permanence of laws,' is accepted on inductive grounds. On p. 196 of the above book Mr. Russell says:

[1] *Outlines of Evolutionary Biology*, 1912, p. 191. Note the 'teleology' in the above passage. This 'explanation' *presupposes* memory.

[2] For a useful discussion of causation see J. Venn, *Empirical Logic*, London, 1907, pp. 46-73. For older discussions see Mill's *Logic* and Bain's *Logic*, Part II. Among the most important recent references on induction are: J. M. Keynes, *Treatise on Probability*, London, 1920; J. Nicod, *Le Problème Logique de l'Induction*, Paris, 1924; and H. Niebet, *Foundations of Probability*, Mind, N.S., Vol. XXXV, p. 1, 1926. See also Part III of Mr. W. E. Johnson's *Logic*, Cambridge, 1924.

> 'The ground of this principle is simply the inductive ground that it has been found to be true in very many instances; hence the principle cannot be considered certain, but only probable to a degree which cannot be accurately estimated.'

And on the same page he adds:

> 'The assumption that *all* laws of nature are permanent has, of course, less probability than the assumption that this or that particular law is permanent; and the assumption that a particular law is permanent for all time has less probability than the assumption that it will be valid up to such and such a date. Science, in any given case, will assume what the case requires, but no more.'

Now on Mr. Russell's view a difficulty arises regarding how we are to recognize a causal law, when we have got one, and how it is distinguished from a 'mere' observed sequence, if at all. Has *any* regularity of sequence a claim to the title of a causal law? Or is there some mark by which we can distinguish the latter from a 'mere' sequence?

In regard to these questions modern philosophers are not at all helpful. All that they have to say seems to be of an entirely negative character. Mr. Russell does not deny the title of 'cause and effect' to the case of night and day, and he also tells us that of innumerable hooters which sound at twelve o'clock and are followed by the departure of workmen from a factory for dinner, one hooter has as much 'right' to be called 'the cause' of the departure of the workmen as any other.[1] Dr. Broad interprets this to mean that a hooter in Manchester which sounds at twelve o'clock has as much 'right' to be called the cause of the workmen in London leaving the factory as any other hooter. Dr. Broad himself does not relish this result. He says:

> 'My own view is that I do not mean by "causation" *any* kind of regular sequence; but that certain kinds of regular sequence are fairly trustworthy signs of the presence of the causal relation. But the final result is the same.'

Dr. Broad also adds that:

> 'the missing factor seem
> tinuity between the sequ

[1] See *Analysis of Mind*, p. 96. In his later books (*Analysis of Matter* and *Outline of Philosophy*) Mr. Russell makes the causal theory of perception, involving the assumption of unobserved causes, the keystone of his doctrine of knowledge, in spite of the fact that in the *Analysis of Mind* he says (p. 98): 'the notion of "cause" is not so reliable as to allow us to infer the existence of something that, by its very nature, can never be observed.'

it is the absence of such continuity between the blowing of the Manchester hooter and the movement of the London workmen which makes me so certain that the former is not a cause of the

But how is this any way of out the difficulty? Surely there is as much spatio-temporal continuity between London and Manchester as between one part of London and another. Moreover, we regard certain terrestrial occurrences as being causally related to siderial occurrences, and there does not appear to be much to choose between the earth and the sun, on the one hand, and Manchester and London, on the other, in the matter of spatio-temporal continuity. The 'missing factor' cannot be *only* spatio-temporal continuity. It seems to me more probable that it is spatio-temporal continuity plus *continuity of change of character*. The average man of science (if he has not been 'debauched with learning' by reading Mach or Pearson) would, I think, reject all the hooters except those between which and a given workman it was possible (at least theoretically) to trace not merely spatio-temporal continuity, but also continuity of change which travelled from the former to the latter. If the two changes are to be considered as causally related the spatio-temporal interval must (he would feel) also be occupied by changes which are causally related. For the same reason the average biologist would reject Mr. Russell's 'mnemic causation', which involves a kind of 'actio in distans' in time. The only scientific writer with whose works I am acquainted who has questioned this principle of continuity is Professor P. W. Bridgman who, in his *Logic of Modern Physics*,[2] appears to have the courage (if I have not quite misunderstood him) to challenge the universal belief that light is 'a thing that travels'. This is characteristic of the revolutionary times in which we live, and makes one feel that biological thought is still in the Middle Ages.

Now the central theme of philosophical criticisms of causation is that a 'causal nexus' is not observed, but that all that can possibly be observed is regularity of sequence. Mr. Wittgenstein says that belief in the causal nexus is superstition.[3] But there is one point which may have some bearing

[1] *The Mind and its Place in Nature*, London, 1925, p. 453.
[2] See the highly interesting remarks on p. 164.
[3] *Tractatus Logico-Philosophicus*, London, 1922, § 5. 1361.

on the apparently ridiculous positions to which this view seems to lead. This view is usually based upon theories of perception of the phenomenalistic type, in which attention is paid to sensa to the neglect of everything else. If these theories of perception are inadequate, then just those respects in which they are inadequate may be important for the understanding of our belief in the causal nexus. It is generally admitted that the scientific discoverer is guided in his observations not by discursive processes but by what is called 'insight', 'scientific intuition', and so forth, about which nothing is known. In other words, there is an 'incalculable element' in scientific research, just as there is in artistic composition of all kinds. It is this which 'makes the difference' between the genius and the ordinary mortal, and it is this which makes it impossible to lay down rules for conducting scientific investigations. But if this is admitted we must also admit that our theories of perception are incomplete. They omit something important. They take account only of the more obvious features of cognitive processes, omitting the subtler and rarer ones. And in the present state of knowledge it would be surprising if this were not so. Accordingly such theories of perception can hardly provide a basis for dogmatic denials. Thus the dispute regarding the causal relation appears to be of the same type as that regarding the 'external world' and our apprehension of both seems to be on much the same footing.[1] The scientific investigator is guided by the belief that natural entities are so related to one another that what happens in one is dependent upon what happens in another. They are dependent upon one another in respect to types of change of character, and two or more things which are thus dependent with respect to one type of change may be independent with respect to a different type, or with respect to different correlated characters. In living organisms the part-events are so related that this 'sensitiveness' with regard to change of character becomes especially acute.

But the scientific worker is apt to sharpen his demands too much, and unconsciously turns his causal postulate into an absolute metaphysical principle. And it is in this form that our notions of causation appear to conflict with certain

[1] Prof. Whitehead has discussed 'causal efficacy' in his *Symbolism*, Cambridge, 1928, p. 35 *et seq.*

biological facts and doctrines. If we conceive the world as 'governed by unchanging laws' it is difficult to see how evolution is possible. For, as we saw in the last chapter, evolution appears to require *change* in the 'regularities of change', or at least the coming into being of *new* types of change. And these, on their first appearance, will be *unique*. But if evolution has consisted in a succession of unique changes then it is clearly meaningless to speak of a 'law of evolution' or of a 'cause of evolution' because, as we have seen, causal laws and the causal postulate do not deal with unique changes.

Now this may also apply to 'variability of response'. It is evident that we can only study what I have called 'biological responses' in causal terms when there is a 'mechanism' already established 'for' them. Their first origin obviously cannot be so studied. Driesch illustrates the difficulty of dealing with variability of response by his imaginary example of the dog crossing the road to get a bone (1) directly without complication; (2) on another occasion involving avoiding a passing carriage; (3) after injury to part of one cerebral hemisphere; and (4) after amputation of one leg. Driesch says:

> 'The concept of the contingency of single motor acts embraces the fact of their modifiability. But as our mind is forced to conceive all that happens as being univocally determined, the problem at once arises, by what factors or conditions the actual performance of a particular movement in a particular case is actually determined as such.'[1]

But is not this an illegitimate use of the notion of 'determination' from the scientific standpoint? We cannot investigate particular cases causally at all *in their particularity*. All we can do is to perform such experiments on a number of dogs and compare them. No hypothesis we might make in regard to a particular case could *possibly* be verified. It is the story of the bishop and the orange peel over again. There is nothing to prevent us from ascribing the fall to direct diabolic or other agency but nothing is thereby accomplished. And in connexion with the application of general physical laws to particular biological changes it will be useful to call attention to the following remark of Dr. Broad:

[1] *Science and Philosophy of the Organism*, 1908, p. 21.

> 'Observation and experiment do no doubt tend to prove that, once started, the changes in a living body obey the laws of motion and the conservation of energy. But both these principles are merely negative; they do not by themselves determine either that a change will happen or when it will happen if it does at all.'[1]

This aspect of 'teleology' then, cannot be dealt with causally. From the causal point of view if a particular occurrence is 'useful' to an organism this is, as we have seen, an 'accident'. But in another sense 'teleology' presupposes 'mechanism'. It is obvious that adaptations of the static or 'stagnant' type are reflections in the organism of persistences or regularities of the environment. Their usefulness or teleological character presupposes this. Yet many biologists seem to suppose that an enduring organic character ceases to be 'purposive' when its mechanism has been discovered. But clearly in such cases it is because it *is* a mechanism that it is useful, so long as the environmental character of which it is a 'reflection' persists. In this aspect there is no antithesis between teleology and mechanism. They simply represent two ways of regarding the same feature of the organism, two ways which (according to the distinguished physiologists quoted at the beginning of this chapter) are both equally important in physiological research. Professor R. B. Perry, who has recently written on this antithesis in the light of modern ideas, says:

> 'As a matter of fact it is not at all necessary to suppose that teleology is the contradictory alternative to some other hypothesis such as "mechanism". The party of teleology insists upon novelty and irreducibility, especially among the phenomena of life and mind; while the party of mechanism proclaims the doctrines and methods of scientific orthodoxy. But there is a growing and justifiable conviction that the hostile rivalry of these parties is based on a misunderstanding.'[2]

On a later page he says:

> 'Life is not purposive by virtue of being emergent or organized; but organization is purposive in the particular case of life by virtue of certain special properties which emerge.'[3]

It seems probable that the antithesis between teleology and causation depends very largely upon three factors: (1) in-

[1] Proc. Aristotelian Soc., Vol. XIX, p. 121. Physics and chemistry do not ordinarily concern themselves with where and when things happen, but this is important in biology, and in this important respect biology differs from the former sciences.

[2] R. B. Perry, *General Theory of Value*, New York, 1926, p. 151.

[3] *Ibid.*, p. 155.

sufficient analysis of these notions themselves; (2) too great haste in trying to bring them into relation; and (3) assuming them to be mutually exclusive. But the only real danger to be feared lies in being too easily satisfied with the belief that the last word has been said on this topic.

Appendix to Ch. X: Summary on Explanation in Biology

Now that we are approaching the end of our task it will be useful to summarize what has been said about explanation from the standpoint of the requirements of biology. And first, having considered teleology a little, we are in a position to add something more to what was said in Chapter VI about the relative merits of the various kinds of mechanical explanation. There it was noted that the recognition of the nature of biological organization leads us to the conclusion that of these only the 'machine theory' (as there defined) is at all adequate to deal with the organism as a whole, because it was the only form of this type of explanation which does not abstract from this organization. In the light of what has since been said we can now make a fuller comparison between the organism and a machine. Organisms *differ* from machines in the following respects:

(1) They are such that their parts are different in their properties when they are separated from the whole from what they are in the whole. Hence, 'being part of a living whole' is an *internal* organizing relation.

(2) The mutual relations of the parts in the whole and of the latter to the environment are such that in the typical environment in which an organism usually occurs in nature it continues to persist 'in spite of' change in the environment 'by means of' change in itself.

(3) Organisms are not *known* to be dependent for their existence on any human mind, or on any other mind. Hence an organism does not bear a relation (in nature) to human needs, wants, or purposes, in quite the same way as is the case with machines, i.e. they are not 'made' for such purposes by man.

(4) A given organism is the outcome of an evolutionary process in the biological sense. (Interpretations of evolution by analogy with the so-called evolution of machines involve, in spite of their popularity, putting the cart before the horse).

(5) Organisms are genetically related to one another.

On the other hand, organisms *resemble* machines in being organized above the chemical level and, as already pointed out, it is for this reason that machines offer an analogy with organisms. But this is also the case with many other things: The solar system, crystals, atoms, works of art and all manner of artifacts. It is because all these entities are single individual organized things that they have all from time to time been appealed to as offering an analogy with organisms. Thus there are three fundamental kinds of 'wholes' or 'organized single things';

(1)	Inorganic.	a. Perceptible: crystals, solar system.[1] b. Imperceptible[2]: atoms, molecules.
(2)	Biological.	Living organisms.
(3)	Artifacts.	a. Artificially synthesized organic compounds. b. Machines, tools and all kinds of human constructions. c. Works of art. Social institutions.

The mode of organization in these different entities varies very greatly, and it seems to be true to say that those under (2) are existentially dependent upon those under (1), and those under (3) similarly depend upon those under (2). Thus mechanical explanation in the 'physico-chemical' sense (as defined in Ch. VI) means interpreting (2) in terms of (1), and therefore involves passing from a lower to a higher type of organization, whereas mechanical explanation in the sense of the machine theory involves passing from a higher to a lower type of organization. A third alternative would be to study the organization of type (2) as a mode of organization *sui generis*, expressed in terms appropriate to it. In this way it would be possible to compare the laws appropriate to this type with those of the other types. Thus to our list of six *reasons*[3] for believing that an *exclusive* attention to what

[1] 'The Solar System' can hardly be called 'perceptible' as a whole, but it is 'macroscopic'.

[2] These, being hypothetical, only 'exist' in nature in a 'Pickwickian sense'.

[3] See above, p. 317 *et seq.*

are called mechanical explanations in biology is not desirable, bearing in mind that vitalism is not the only alternative' we can add a seventh, which may be described as 'depending on the fact that living organisms exhibit (in some measure at least) "internal teleology" in the sense above explained (p. 436).'[1]

We now have to turn to the task of making a general summary of the more important points regarding explanation which have emerged in the foregoing discussions. It is evident that the term is used in very different senses. In the first place we have noted the common-sense usage in which explaining something simply means bringing it into relation with the familiar. In the Introduction it was pointed out how inadequate this account of the matter was from the scientific standpoint. What we now have to do is to discover what is essential in scientific explanation, and to classify the kinds of explanation employed in the biological sciences. I must confess that I do not understand what is meant when people say that science does not 'really explain' anything, because I am incapable of understanding the subtle difference between 'explaining' and 'really explaining', and the people who appear to make such a distinction never pause to state what they themselves mean by it. I am content with trying to discover what is meant by 'explaining'. Also it is not quite clear what is intended by saying that explaining in science is only description. The celebrated saying of Kirchoff is often quoted in this connection. According to this the aim of science is to describe the occurrences of nature completely and in the simplest fashion. But taken literally this would be impossible and quite useless. In the ordinary sense of the terms it would be impossible to describe anything completely, and such a description, even if it were possible, would be entirely without scientific value. And to call a set of differential equations which represent certain abstract features of some physical process a description is obviously to depart from the ordinary use of that term.

It was pointed out above that in all cases of explanation we proceed by a combination of analysis and relating. We might therefore base our classification of explanations in the first place on the epistemological status of the *relata* involved

[1] It would doubtless be desirable in biology to avoid the term 'teleology' if a suitable substitute could be found.

and then we could further divide them according to the intrinsic properties of the relata and according to the *relations* involved. In this way we should have two primary kinds of explanation which we can call (1) Perceptual, in which the relata are perceived entities; and (2) Conceptual, in which the relata are not perceived entites. Now in biology in the strict sense (i.e. where we are dealing with entities above the chemical level of organization) most of our explanations belong to the first type in the form of so-called empirical laws. And it is worth while mentioning in passing that it is a mistake to despise empirical laws of this kind, and to regard them as something inferior as compared with the so-called exact laws of physics which involve hypothetical imperceptibles. It is useful to note what Mr. Bertrand Russell has said on this point:

> 'Laws embodied in differential equations may possibly be exact, but cannot be known to be so. All that we can know empirically is approximate and liable to exceptions; the exact laws that are assumed in physics are known to be somewhere near the truth, but are not known to be true just as they stand. The laws that we actually know empirically have the form of the traditional causal laws, except that they are not to be regarded as universal or necessary. "Taking arsenic is followed by death" is a good empirical generalization: it may have exceptions but they will be rare. As against the professedly exact laws of physics, such empirical generalizations have the advantage that they deal with observable phenomena. We cannot observe infinitesimals, whether in time or space; we do not even know whether time and space are infinitely divisible. Therefore rough empirical generalizations have a definite place in science, in spite of not being exact or universal. They are the data for more exact laws, and the grounds for believing that they are *usually* true are stronger than the grounds for believing that the more exact laws are *always* true.'[1]

Now under the second head of 'conceptual' explanations will come these ideal mathematical laws of physics when they involve imperceptibles, and it is important to remember, as was pointed out in the Introduction, that the use of such imperceptibles has been double sided. On the one hand there has been the representation of such imperceptibles in the imagination on the model of billiard-balls, etc., and on the other hand there has been the process of calculation which is really independent of the imagined particles, and still survives whatever changes take place in current views regarding the latter. In modern physics the latter tend to drop out,

[1] *The Analysis of Mind*, London, 1921, pp. 93-6.

e.g. in the latest developments of atomic theory. Accordingly conceptual explanations are divisible into two kinds: (1) those accompanied by calculation; and (2) those not accompanied by calculation. In the first case we have generalization and predictability, and a high degree of abstraction. In the second case we have an explanation in the sense that *if* the imagined particles existed, and *if* they had the properties which are assigned to them, we could understand how the observed result is reached. But if nothing more is done than to 'account for' what is observed by a purely hypothetical construction invented *ad hoc* for the purpose, then we have no genuine explanation at all but a purely circular procedure. This has very commonly occurred in biology in the past. Instances are furnished by Darwin's theory of pangenesis and Weismann's doctrine of the architecture of the 'germ-plasm'. Mock explanations of this kind often have the merit of stimulating research, but they also have the unfortunate disadvantage of creating the impression that something is understood when it is not, and they are consequently apt to have a blinding influence.

It is by a comparison of these mock explanations with successful ones that we are able to understand what exactly it is which constitutes the essence of a scientific explanation. That essence is the *systematization* of knowledge. It is because the unsuccessful explanations do not lead to generalizations and thus to systematization beyond the field for which they were devised *ad hoc*, that they do not constitute explanations. Consider, for example, the work of Koch and Pasteur in relation to the systematization of medical knowledge. Before their time bacteria were known, and there was an enormous amount of medical knowledge of a sort. The merit of the work of Koch and Pasteur did not lie in the fact that it assigned 'the cause' to a particular disease, but in the fact that the systematic correlation between a particular specific parasite and a specific complex of symptoms was of such a type that it could be generalized over the vast majority of diseases, only a very few at the present day being exceptions to it. Moreover, the work of Pasteur also provided the key to the then little known sphere of immunity phenomena. In this example the relative roles of new facts and new concepts are very completely interwoven, but the essential point is that a new system of knowledge was thereby created.

CHAPTER XI

The Antithesis between Mind and Body

So far in this book we have more often been concerned with the relations of biology to the physical sciences, to which biologists have looked for aid in dealing with biological problems, rather than with the relations of this science to psychology. But we have also noted a tendency on the part of some biologists to look also to psychology for aid. Biological writers differ greatly in their attitude towards psychology. Some, as we have seen, are for 'reducing' biology to physics, and among these some draw a sharp line when it comes to psychology and are in favour of leaving psychology to its own devices. Others, on the other hand, appear to believe that psychology 'reduces' to physiology, and that in the long run means, presumably, to physics. Those who take this view are prepared to tell psychologists their business, how they must conform to this or that physiological fact, and so forth. And then there are those who believe that biology does not reduce to physics and are prepared to borrow notions derived from human psychology and extend them to the lower animals. Thus the old quarrel between what we may call physical biologists and biological biologists (mechanism and anti-mechanism) breaks out again, when we come to the relations of biology to psychology, between the physiological psychologists and the psychological psychologists. All these quarrels seem to spring from the deep-seated desire for monism which burns in so many scientific breasts, coupled with the dregs of eighteenth and nineteenth century traditions.

I shall not attempt to discuss these quarrels in detail even from the methodological point of view, still less is it necessary to go over the well-trodden ground which occupied the centre of interest for the Victorians. The ontological problem as it appeared to them and the various metaphysical 'solutions' which they quarrelled over wear a different aspect to-day,

and in any case this is not the place to discuss such aspects of this antithesis. All I propose to do is to explain as clearly as I can why, for epistemological reasons, the relations between biology and psychology are of a totally different nature from those between biology and the physical sciences, and then to point out some of the consequences of this difference.

In the last century these debates took the form of discussions about the relation between 'mind' and 'matter', the assumption frequently being made that there was no difficulty about what these terms meant, but only about the relation between the entities referred to by them. And, of course, for many purposes everyone does know roughly what he means when he uses these terms. I have used the term 'mind' in the present book in various places assuming that for the purpose there involved the reader would understand sufficiently clearly what was intended. But when we come to the primary metaphysical problem of the relation between 'mind' and something else then it does not suffice to employ such expressions without further analysis. Instead of beginning in this traditional way with vague abstract substantives, it would be much better to begin with facts that we know, however much dispute there may be about the precise interpretation of them. If we do this we find that, so far as the present problem is concerned, there are two principal types of facts involved. In the first place, there are facts of the type we assert in the propositions: 'There is a jug on this table'; 'This frog has four legs'; and also in universal propositions, such as: 'All brains consist chiefly of nerve-cells and processes'. It is to this type that the propositions of natural science (as defined in the Introduction) belong. But there is another and quite different type of fact which we assert in such propositions as 'I am now seeing a red patch'; 'I am now remembering a telephone number'; 'I am now thinking about the next general election'. I do not think anyone denies that there *are* facts of this kind, and when people suppose themselves to be denying them they are really denying certain *theories* about them. Facts of this type are evidently of a different nature and do not form part of the subject-matter of natural science (as here understood).

Now one obvious difference between these two types can be expressed by saying that the first are *public* facts, and the second are *private* facts. If it is the case that 'a jug is on

this table' there is no theoretical obstacle to the possibility of any number of people knowing it. But if it is the case that 'I am now remembering a telephone number' it is not possible for anyone else to know it unless I choose to tell them, and even so they cannot be certain that I am telling the truth. Thus in the second case the facts are 'strictly private' to one knower, and other knowers can only know them at second-hand, and with some element of uncertainty.

Correlated with this difference of accessibility of the two types of facts is the difference in the way in which they are known. Facts of the first type are known through sense, those of the second are not—they are only known 'immediately' or 'introspectively'.

Now the antithesis between 'mind' and 'body' rests upon the existence of these two types of facts, and on the difficulty of bringing them together on to the same epistemological footing. If this were possible there would be no need for speech, and even with speech we are no better off in regard to the present question, because speech does not allow us to *introspect* private facts relating to other people, and introspection is the only way of knowing private facts *directly*.[1] We have no means of passing from neurological public facts to private facts. The only way in which we could establish a correlation between the facts of these two types which are relevant to the present antithesis would be by introspecting our own private facts, and at the same time sense-inspecting those public facts of our own brain with which they are commonly assumed to be correlated. If this could be done, and if it could be established that there is a precise one-one correlation between every particular private fact and some particular public fact, we should then have a means of passing from public facts to private facts. But since this is just what we cannot do, all that remains for us to do is to speculate about this relation, or, what is more popular, to dogmatize about it.

This being the state of affairs what methodological consequences flow from it which are of importance for biology and its relation to psychology? It seems clear to me that there is no escape from the conclusion that as far as natural science is concerned the only correct attitude is behaviourism—so long as we are dealing with organisms which are incapable

[1] Telepathy appears to be too rare and sporadic in its occurrence to require any modification of the above statement in its favour.

of asserting propositions. In other words, it is quite impossible to study private facts relating to the lower animals—we cannot even *know* whether there are any. If we are to adhere to what is verifiable—as natural science at its best always has done—then there seems to be no sound methodological alternative to behaviourism. If by psychology you mean the study of private facts there can be no comparative psychology. And it follows from this that we can make no scientific assertions about the evolution of 'mind'. If we attempt to introduce, among public facts relating to organisms, private facts which we know only in relation to ourselves, how is our procedure to be distinguished from ontological vitalism? From the methodological standpoint one is just as inadmissible as the other. You are simply introducing private facts in order to 'eke out the collapse' of an explanation in terms of public facts. And when we come to man, who *is* capable of 'asserting propositions', we are really no better off. It is just as illegitimate to introduce private facts into an exposition of human cerebral physiology as it is in the case of lower animals. Public facts of this kind and private facts refuse to mix.

But it does not follow from this that a study of private facts, even in other human beings than ourselves, is impossible or valueless. Here I should disagree with the extreme behaviourists who are not content with their own methodology but, as is the way with human beings, must carry the war into what they erroneously suppose to be the enemy's camp, and so set up an 'antithesis'. The success of the psycho-analytical procedure shows that it is possible to study private facts scientifically. But the success of this procedure has depended on the fact that they have done what the behaviourists have done—they have refused to mix their facts with the other type of facts. So long as you adhere to public or to private facts and keep them pure, all goes well—you remain all the time on one epistemological plane. If this is done the behaviourist need have no quarrel with the psychologist. His is the easier task because public facts are directly accessible and afford a safer basis for generalization. But he is in no position to throw stones at those who choose the more difficult task. A theory relating to the private facts of a given individual *can* be verified *for that* particular individual in spite of the difficulties.

a man's brain when he is looking at and touching a box one can understand that by fusion may be meant some sort of combination of afferent neural impulses. But this cannot be discovered by introspection, for one is certainly not aware of any sticking together of 'sensations'. It seems clear from this that by perception Dr. Bolton does not understand a private fact, as a psychologist would, but a public one, although he seems to use the same terms for both. And on account of this double use of terms there seems to be more confusion in Dr. Bolton's article (which is all in the same vein) than in a whole library of behaviourism. Neurologists persist in mixing up three things: (1) sensa, which they usually call either 'sense-impressions' or 'sensations'; (2) neural impulses, which are public facts, although not *directly* observed ones; (3) private facts expressed in words ending in -ing; e.g. thinking, perceiving, sensing, remembering, etc.

The same type of confusion is also illustrated by the following passage by Professor Elliot Smith:

> 'The ability to learn by experience necessarily implies the development, somewhere in the brain, of a something which can act not only as a receptive organ for impressions of the senses and a means for securing that their influence will find expression in modifying behaviour, but also serve in a sense as a recording apparatus for storing such impressions, so that they may be revived in memory at some future time in association with other impressions received simultaneously, the state of consciousness they evoked, and the response they called forth.'[1]

The term 'impression' occurs three times in this passage. On the first occurrence it seems to mean 'neural impulse' but its significance on the other occasions seems doubtful. If the second use of the word means neural impulse it is again difficult to understand what is meant by saying that these are 'stored', and if it means sensa how do we know that these 'call forth' responses? Moreover, if it means sensa we have a case of 'introjection' (p. 94) and all its consequences. Could not all these dubious points be avoided by adhering to the behaviourist's point of view and terminology, and is anything really *gained* by bringing psychological terms in? A similar difficulty is raised by the following passage:

> 'The power of discrimination which resides, so to speak, in this sensorium, and is fed by the continual stream of sensory impressions pouring into it and awakening memories of past

[1] *Essays on the Evolution of Man*, Oxford, 1924, p. 31.

> experiences, can express itself directly in the behaviour of the animal through the intermediation of a part of the neopallium itself, the so-called motor area.'[1]

What is meant by a power of discrimination 'residing' in the neopallium? Note that the author in both passages uses such expressions as 'in a sense' and 'so to speak' indicating the recognition of a difficulty. In speaking of discrimination we may mean, as far as I can see, one of two things. We may mean discrimination in a psychological sense as when we speak of discriminating red from blue. Or we may mean discriminating in the sense in which an automatic machine can be said to discriminate between a penny and a half-penny. If in the above passage is meant discrimination in the first sense then the author has imported what can only be discovered by introspection into what he has discovered in brains by means of sense-experience.

A very good example of what seems to be simple animistic explanation in neurology is afforded by the following passage from another neurologist. Professor C. J. Herrick is explaining how a catfish takes its food:

> 'Food may be felt through the V nerve and tasted through the VII nerve. If it feels right and tastes right the two stimuli co-operate, the motor V and motor VII nerves are activated, and the food is held and ultimately swallowed. If it feels right but tastes vile the motor V is inhibited, the mouth opens, the motor VII is activated, and the food is ejected by the tongue. If the feel and the taste are extremely noxious, the nervous impulse also passes down to the spinal cord and the fish swims away.'[2]

Professor Herrick adds that 'This is not a hypothetical case; it is well authenticated'. But does this refer to the public facts observed or to the interpretation? If to the latter then the fish must have been consulted, since no one else knows what tastes 'right' or 'vile' to fishes! Thus in this case the nerve-cells of the fish (or its 'ego') are permitted to make *judgments of value* and it is these which 'explain' what path the nervous impulse takes. It is just this which constitutes the fundamental problem—how it is that a particular linkage in the nervous system is established on a particular occasion, and it seems doubtful (as was suggested in the last chapter) whether this problem can be studied scientifically at all. If we interpret it in the above way are we not unconsciously

[1] *Op. cit.*, p. 32.
[2] *Brains of Rats and Men*, Chicago, 1926, p. 39-40.

appealing to vitalism? Professor Herrick himself says on the same page that 'What happens in each case seems to be determined by the relative potencies of the several members of the complex.' This is certainly vague, but it expresses a mode of viewing the situation which would seem to make any reference to whether the food feels or tastes 'right' unnecessary. It is also perhaps worth mentioning that even in our own case when we make judgments of value regarding gustatory matters the 'I' that judges knows nothing about the linkages in its nervous system, so that even in this case such judgments will not help us in a physiological explanation. I hope these few examples will illustrate the contention brought forward above that nothing is gained by mixing private facts with public facts in neurology.

It seems plain that the adjectives 'physical' and 'mental' express the difference between public and private facts, and the terms 'matter' and 'mind' exemplify the use of the category of substance in relation to these two types. Matter, minds and the causal nexus seem to be three entities 'not observed by us' (according to ordinary accounts) which are nevertheless very useful in systematizing our experience. For reasons already given there is no occasion here to discuss the relative merits of the various metaphysical theories which were popular in the last century regarding the 'petty question' of the relation of mind to body which, according to Whitehead, has been confused with what he calls the 'grand question' of the relation of mind to nature. I should feel inclined to agree with William James that they are but 'spiritual chloroform'—devices for 'making a luxury of intellectual defeat'. But it is desirable to protest against their admission into theoretical biology, especially when their metaphysical nature is not made clear and they are allowed to slip into the exposition as respectable scientific theories. At the present moment speculations of the 'double-aspect correlation' type appear to be popular among biologists. I say nothing about the merits or demerits of these speculations from the standpoint of the metaphysician but I am merely concerned to show as plainly as possible their extra-biological nature, in order to hasten the process of differentiation of metaphysics from natural science. An example of the introduction of speculations of this kind into biological expositions is found in a little book by Professor E. S. Goodrich which is intended for the

'general reader'. After a solemn warning against trespassing into the 'region of philosophy' and after urging the reader not to confuse mechanistic biology with 'materialism' which, says Professor Goodrich, is 'a discredited system of philosophy which denies the existence of anything but the material or physical,' there follows what appears to be an exposition of psycho-physical parallelism of the type referred to:

> 'We believe that to every mental process, whether of the "highest" kind in the mind of man or of the "lowest" in that of the most primitive organism, there corresponds some physico-chemical change. How intimately connected are the two sets of processes is a matter of common knowledge. The lightest disturbance or interruption in the metabolism of the brain, due to an injury, an anaesthetic, a poison, will have its echo in a disturbance of the mental activities.'[1]
>
> 'So far as we know, neither the mental nor the metabolic processes can take place without the other. Yet, indissolubly bound together as they are, the one is certainly not the product of the other, nor can it interfere with the continuity of the other.'

Such confident assertions are likely to give the general reader an unfortunate impression. If he knows anything about logic and metaphysical speculation he will know that this is not biology and that there is not much of this that we can be said to 'know', still less that we can speak with 'certainty' about it. He may then be led to draw unflattering conclusions about the incautious use of such speculations by biologists. But if he is ignorant of these things he may be led to believe that we know a great many things which we do not know and perhaps never will know. On p. 176 the reader is expressly told that the student of biology 'should realise that the mental series of events lies outside the scope of natural science' and yet in spite of this and of the warning not to 'trespass into philosophy' we are told that the 'physico-chemical' and the 'mental' processes are 'two aspects of one and the same series of events', and 'two abstractions from the same fundamental reality'. Is this biology or is it trespassing into philosophy? Probably the least sophisticated reader will understand that when an author begins to talk about 'fundamental reality' he has ceased to talk science. Would it not therefore be better to omit such speculations in a biological book or at least to say quite frankly that such things belong to metaphysical speculation? And would it not be only just to point

[1] *Living Organisms*, Oxford, 1924, p. 174.
[2] *Ibid.*, p. 175.

out also that this 'fundamental reality' is a hypothetical entity *postulated* to provide an ontological filling for the gap resulting from our epistemological dualism? On p. 173 Professor Goodrich writes: 'We know of no mind apart from body, and we have no right to assume that metabolic processes, can occur without corresponding mental processes, however simple they may be'. But equally we have no right to assume that metabolic processes *cannot* occur without corresponding mental processes. We cannot observe metabolic and mental processes together at the same time in the same way and consequently we have no way of deciding by observation between these two alternatives. But the situation is even worse than it appears because we discover later in the book that Professor Goodrich makes a distinction between 'conscious' and '*unconscious*' mental processes. Conscious mental processes are apparently the ones we observe in relation to ourselves, and unconscious ones cannot be observed at all but are mere postulates to enable the evolutionist to get over the difficulty of the evolution of 'mind'. But what is meant by mind and how is it known to have evolved? There are obviously a number of alternative answers to these questions none of which can be subjected to any test which would be recognized by natural science. Let us consider some of them.

What we observe through sense experience appears to be organized systems of sensa in relation to places during periods of time. Some of these systems have an enduring character so that they can be recognized and called perceptual objects. Some are called living organisms, and what are called metabolic processes are certain abstract recognizable types of change characterising events which are parts of organisms. What we observe by introspective experience and call mental processes appear to be very different entities from sensa or spaces and times. In sensing a red patch I am aware of the red patch and I may also be aware that I am sensing a red patch but it is very difficult to say anything more. If such acts of awareness and of introspection are to be called conscious mental processes, and if it is so difficult to find out anything about them, what am I able to say about hypothetical unconscious ones? And when I am being aware of a red patch what do I know about any metabolic processes which may be going on at the same time which would enable me to

say that I have no right to assume that such processes cannot occur without corresponding mental ones? Then again: am I to give the name 'mind' to the series of mental processes which we call sensing, perceiving, remembering and so on, or is it to be given to something else—an ego or self or subject which is said to 'have' or to 'perform' these processes? If the latter alternative is chosen how do I come to know that there is such an entity and what is its relation to the mental processes? When it is said that mind has evolved is this to be understood to apply to the mental processes or to the ego or subject? It is evident that there are many possibilities between which it is quite impossible to decide by any empirical test, and it is still more evident that no answer can be given to such questions from the standpoint of biology. I conclude then that it is desirable to avoid confusing these problems with biological ones, because it only tends to mislead people into supposing that our biological knowledge is of a different character from what is actually the case.

A word of explanation is perhaps necessary in reference to what was said in Part I. about 'percipients' in relation to theory of knowledge. An examination of knowledge seems to require (1) percipients or knowers; (2) that of which percipients are from time to time aware; and (3) knowledge, which in natural science is a system of propositions about (2). But I deliberately avoided making any ontological assertions about (1) and (3) beyond what was or seemed to be required for describing the situation of perception from the epistemological standpoint. Similarly when I said in a previous chapter that 'being possessed of minds' seemed to signify a new mode of persistence for human beings as organisms in virtue of the possibility of prevision, this statement is not made from the standpoint of biology but from common sense, and I therefore put the above expression in inverted commas to indicate that it was an unanalysed one. But the correct analysis of this expression cannot be undertaken from the standpoint of biology. From the common-sense standpoint we all 'know' vaguely what it means, but from a critical point of view we cannot be said to know what it means. And from the metaphysical standpoint there are a number of alternative analyses between which it is impossible to decide. Any biologist therefore who introduces one possible analysis into a biological discussion without understanding and making

clear its true character is guilty of confusing natural scientific knowledge with metaphysical speculation.

It will be noticed that in the passages quoted from Prof. Goodrich no reference is made to sensa. One would be led to suppose that the world can be exhaustively analysed into chemical and physical processes and mental processes, these being 'aspects' of the hypothetical fundamental reality which is not observed. But if this is so what becomes of sensa which *are* observed? Are these physico-chemical or mental? According to Prof. Lloyd Morgan they are mental, but this is a pure assumption for which no specially compelling reason is given. Why should everything be either physico-chemical or mental? There seems to be no very good reason for supposing that sensa are either, and no very good reason for supposing that everything which belongs to 'nature' can be exhaustively described as physico-chemical. It would in fact be very difficult to give a precise meaning to the expression physico-chemical, and it is just because so little attention is given to precise meanings that so much of our so-called knowledge is seen to be no knowledge at all as soon as it is submitted to strict analysis. Anyone can convince himself of this who will sit down to an honest attempt to discover the precise amount of information that is conveyed by the statement that body and mind are two aspects of one fundamental reality. If he conscientiously analyses the meaning of each word he will soon find that they conceal so much ignorance, and involve so many unwarrantable assumptions, and unanalysed notions, that he will be too much occupied with them to feel much confidence about the meaning of the proposition as a whole. Nothing is easier than to utter grammatically correct sentences which appear to say something important but which are found on examination to convey next to nothing. The more one reflects about this question the more one sees how impossible it is to speak of 'our knowledge' concerning it, still less of 'certainty'. But fortunately the biologist as such has no occasion to trouble himself with all these difficulties. His hands are full enough with difficulties and unresolved muddles in his own sphere, and if he devotes his energies to his own affairs he will have little time left for amateur psychology and metaphysics. All he needs to know about these topics is just enough to save him from introducing them into books which profess to confine them-

selves to biology. But it does seem to me that Prof. Goodrich, in the little book referred to, although professing to confine himself to scientific biology, has not been able to resist the temptation of expanding it into a metaphysic of things in general along typical nineteenth century lines. He not only expounds the 'double aspect' theory but he also applies the doctrine of natural selection to ethics, and the consequences of this are interesting and worth pointing out. I have not attempted to discuss the 'antithesis' between Darwinism and Lamarckism in this book because the whole subject at present seems to be in a chaotic condition. But the Darwinian doctrine appears to be a kind of logical blank form waiting for data to fill it in, and for reasons already given these data, so far as changes in the remote past are concerned, are not forthcoming, and consequently it cannot be used as an 'explanation' of such changes. Moreover, I have brought forward reasons for believing that from the peculiar nature of the case no *causal* explanation of evolution is possible.[1] And yet Professor Goodrich says that the historian of biology 'will not dwell with much pride on the account of the criticisms of Darwinian doctrines.' He admits that 'it does not explain everything, that many problems remain unsolved' but nevertheless:

> 'the Darwinian theory still remains unassailable as the one and only rational scientific explanation of evolution by "natural" forces, whose action can be observed, tested, and measured.'

And in an earlier place he writes that:

> 'a complete scientific aspect of the process of evolution can be described as an unbroken series of "natural" events, a sequence of cause and effect, a series of steps each one strictly determined by that which came before, and determining that which follows after.'[2]

It is interesting to contrast this with the following expression of opinion by the more critical Bateson:

> 'The many converging lines of evidence point so clearly to the central fact of the origin of the forms of life by an evolutionary process that we are compelled to accept this deduction, but as to almost all the essential features whether of cause or mode, by which specific diversity has become what we perceive it to be, we have to confess an ignorance nearly total. The transformation

[1] The only biological writer in whose works I have seen this clearly recognised is J. Schultz, see his book mentioned on p. 424.

[2] *Op. cit.*, p. 144.

> of masses of population by imperceptible steps guided by selection, is, as most of us now see, so inapplicable to the facts, whether of variation or of specificity, that we can only marvel both at the want of penetration displayed by the advocates of such a proposition, and at the forensic skill by which it was made to appear acceptable even for a time."[2]

The following is the passage in Professor Goodrich's book in which the doctrine of natural selection is brought into relation with ethics:

> "Too often the Darwinian doctrines are represented as teaching that success in the struggle for existence is obtained only by tooth and nail, by blood and iron. This is a very mistaken view. Brutality, fraud, greed, may secure temporary success; but the triumph of the human race over the lower organisms, and again of the higher civilizations over the lower, has been brought about, on the contrary, through mutual help, co-operation, self-sacrifice. These are the very bonds which hold societies together. Religion, art, and science all play an important part in evolution; and morality appears not as an external force working against a ruthless and immoral Cosmic Process, but as a product of that very process and an all-important factor in its development."[3]

Is this not a great muddle? Did Clive succeed in India by brutality, fraud and greed, or by co-operation and self-sacrifice? Surely it was co-operation with those on his own side but brutality, fraud and greed between his side and the enemy. Also in the late war there may have been co-operation and self-sacrifice between the Allies (although even that has been disputed) but it was shells and big battalions—in other words blood and iron—which determined the issue between them and their opponents. In short it was natural selection of the typical gladiatorial-show type. What is to decide which civilisation is the highest on Darwinian principles? Surely the fittest, and that means the one which succeeds—whether that success is achieved by fraud, cunning, or co-operation is all one from this point of view; the great thing is to succeed. It seems to be but another case of trying to have the best of both worlds to attempt to mingle Darwinism and morality. If honesty is the best policy from the standpoint

[2] *Problems of Genetics*, p. 248. *Cf.* also Johannsen: "In Wirklichkeit ist das Evolutionsproblem eigentlich eine ganz offene Frage." "Darwin wir von dem praktischen Wissen und Lehre seiner Zeit durchdrungen; darum musste seine Selektionstheorie damals berechtigt erscheinen. Jetzt liegt die Sache ganz anders: Eine zeitmässige Theorie der Evolution haben wir augenblicklich nicht!" *Kultur der Gegenwart*, Teil III, Abt. IV, Bd. I, pp. 690-60.

[3] *Op. cit.*, p. 188.

of 'survival value' then we must be honest, but if occasion arises when greed and fraud are best then we must be greedy and fraudulent if we want to succeed and be numbered among the 'highest civilizations'. If you are going to apply Darwinism (so-called) *consistently* in morality there does not seem to be any way out of this conclusion. If the theory is true it implies that whenever we say that a particular mode of conduct is good this is equivalent to saying that it is *expedient*. It implies also that the only value in science, art or religion lies in the fact that nations which practice these things are more likely to succeed in the struggle for existence than nations which do not, not because there is anything valuable in them in any other sense. Finally it would mean that by saying that the Darwinian doctrines are reprehensible *if* they teach that brutality, fraud, etc., are the *only* means to success, *all* that we can possibly mean by this is, not that brutality, etc., are in themselves evil, but that as a matter of fact they are not successful in the struggle for existence. Consequently it would imply that *if* by chance they *were* successful, brutality, fraud and greed would be good and *not* self-sacrifice, etc.

On the other hand, if anyone feels that there is something wrong with this account of morality he has no other recourse but to consider the advisability of giving up the application of the Darwinian theory to the sphere of morals, because as it stands the above is certainly what it *means*. It *may* be true that 'good' does *not* simply mean 'expedient' or 'successful in the struggle for existence', although many people behave as if they believed that it did. But if you believe that good does *not* mean this, and if you believe that human beliefs and scientific doctrines influence human actions, you will not wish to advocate the Darwinian doctrine of morals, however much you may believe in the truth of that doctrine in the biological (as contrasted with the human) sphere. It may be wiser in actual life to be guided, not by speculative abstract theories, but by the good sense and moral intuitions of mankind, halting and feeble though they may be—perhaps only just 'emerging'. We must not forget that our biological theories just because they *are* biological, abstract from what is characteristic of the civilized human level of attainment. The wise man who understands that the intellectual is only one among many other human activities, and that there are other values than biological ones, will, perhaps, follow the second of the

above alternatives. There is always a danger, if we are in too great a hurry to bring abstract speculative theories, about which there is still so much dispute, into practical life, of exposing ourselves to the ridicule of wise men in other walks of life and of discrediting science in their eyes. It is interesting to read what Mr. Bernard Shaw has said on this topic. This is not a criticism of scientific theories as such but of their hasty application to human problems:

> 'But in the middle of the XIX century naturalists and physicists assured the world, in the name of Science, that salvation and damnation are all nonsense, and that predestination is the central truth of religion, inasmuch as human beings are produced by their environments, their sins and good deeds being only a series of chemical and mechanical reactions over which they have no control. Such figments as mind, choice, purpose, conscience, will, and so forth, are, they taught, mere illusions, produced because they are useful in the continual struggle of the human machine to maintain its environment in a favourable condition, a process incidently involving the ruthless destruction or subjection of its competitors for the supply (assumed to be limited) of subsistence available. We taught Prussia this religion; and Prussia bettered our instruction so effectively that we presently found ourselves confronted with the necessity of destroying Prussia to prevent Prussia destroying us. And that has just ended in each destroying the other to an extent doubtfully reparable in our time.'[1]

This is hardly relevant criticism from the scientific or metaphysical standpoint, but it seems to be good common sense, and practical life is the realm of common sense. It is interesting to compare this passage with the experience of an American biologist at German Headquarters during the late war. Prof. V. Kellogg writes:—

> 'I had during 1915 and 1916 a peculiar opportunity of hearing set forth as ably, probably, as the argument can be presented, the reasons which lead some men to believe that war is not only inevitable through all human existence but desirable. Part of this argument came to me with special interest because it was based on grounds of biology and biological law. It came from certain officers of the German General Staff living at German Great Headquarters in Occupied France.'

> 'One of the Staff officers was in civil life a professional biologist of much repute, a professor of zoology in one of the larger German

> 'The argument to which I have referred is based on the assumption that natural selection is the all-powerful factor, almost the

[1] *Heartbreak House*, London, 1927, p. xiii.
[2] *Human Life as the Biologist sees it*, New York, 1922, p. 91.

sole really important factor in organic evolution. And that as man as an animal species is subject to the control of the same major evolutionary factors as control the other animal kinds, his evolutionary progress or fate is to be decided on the basis of a rigid, relentless, natural selection. It is the argument from a post-Darwinian point of view that goes much beyond Darwin's own conceptions.'[1]

'If he (man) decides, as the Germans seemed to, that the best way to develop the highest type of man and human culture is to depend solely on the natural selection based on a ruthless physical life-or-death determining struggle for existence, with a survival and dominance of the physically strongest, then war is desirable.'[2]

These passages illustrate some of the consequences which follow close attention to a restricted range of experience in the interests of scientific specialization. They illustrate once more the 'intolerant use of abstractions' and the result of 'seeking simplicity' without 'distrusting' it. It is evidently taken for granted without question (1) that man is evolving in the *biological* sense; (2) that this human evolution is 'governed' by the same 'laws' (if any) as govern *all* organisms; (3) that there are general universal evolutionary laws applicable to all organisms at all evolutionary levels; and (4) that we know enough about the said laws to be able to make sweeping assertions of the kind contemplated in the arguments to which Professor Kellogg refers. And yet anyone who has reflected at all about the present state of our biological knowledge, especially with reference to evolution, will see how difficult it is to say whether any of the above four assumptions are true. Moreover, it may be doubted whether (1) it is desirable that only the 'physically strongest' should survive, and whether (2) under the conditions of modern warfare they do in fact survive.

It is also assumed that 'man' is in a position to 'decide' how and in which direction he is to develop. But this assumption does not seem to be consistent with the creed of mechanistic biology. On p. 193 of his book Prof. Goodrich says that 'civilized man endeavours deliberately to direct the course of his own evolution.' But he also says that evolution has been a process consisting of 'a sequence of cause and effect, a series of steps each one strictly determined by that which came before, and determining that which follows after.' How

[1] *Ibid.*, p. 54.

[2] *Op. cit.*, p. 61. It is worth while to contrast these passages with that from Loeb quoted in the Introduction, p. 64.

is it possible to reconcile these apparently contradictory assertions? Assuming that man is evolving, is it possible to subscribe both to dogmatic behaviourism and eugenics without contradiction?

I am quite sure I do not know the correct answer to any of these conundrums, and I feel fairly confident that no one else does. But one thing seems *perfectly* clear, and that is that they have nothing to do with genuine biological science, and that biological science is quite incapable of answering them. I have introduced them here to show the need of a better differentiation in the minds of biologists between metaphysical, ethical and biological problems, and to draw attention to the desirability of not confusing methodological postulates with metaphysical revelations. If our scientific education makes people so intellectually myopic as to be incapable of seeing the difference between these things, it is time our scientific education was revised. All these inconsistencies spring from the metaphysical extension of the biological notion of evolution, and the metaphysical misuse of the causal postulate.

And in regard to the application of biological theories to human affairs I should be inclined to take the view expressed by Mr. Bertrand Russell when he says, writing from a more general standpoint:

> 'Only the most abstract knowledge is required for practical manipulation of nature. But there is a grave danger when this habit of manipulation based upon mathematical laws is carried over to our dealings with human beings, since they, unlike the wire, are capable of happiness and misery, desire and ... It would therefore be unfortunate if the habits of mind which are appropriate and right in dealing with material mechanism were allowed to dominate the administrator's attempts at social reconstruction.'

And when we bear in mind the uncertainty and extreme primitiveness of our present biological knowledge (which tends to be obscured by the dogmatic way in which biologists so often present the particular one of the alternative theories to which they happen to subscribe), when we recall how abstract it is from the human standpoint, the dangers of applying it to human affairs appear to be particularly serious, and call for more than ordinary caution on the part of those who are eager, no doubt with the best intentions, to advocate such applications.

CHAPTER XII

The Future of Biology

In this book we have been studying what is commonly called the philosophy of science with special regard to the problems of theoretical biology. But I have avoided this expression because I have been trying to close rather than to widen the breach which separates the natural from the philosophical sciences. These sciences interpenetrate one another. Every branch of natural science has, it seems, three aspects: first and foremost is the investigatory aspect which is concerned with discovery; secondly is what we may call the speculative aspect, whose business it is to discover means for systematizing the knowledge furnished by investigation, issuing in constructive theories. The third aspect is the critical, which has to do with investigating methods of interpretation from the standpoint of logic and epistemology. It seems to me to be quite erroneous to suppose that natural science only has the first aspect because no investigator works or can work without some theoretical background however hypothetically it is entertained. In the minds of most men of science little or no separation is made between the first and second aspects. The same men who make the discoveries are usually responsible for the theories, although this is less true of physics than of biology. It seems perfectly obvious that as biology develops, and its several branches become more and more specialized, the more difficult will it be for an investigator to obtain a sufficiently wide and detached view of the whole field to make inductive generalizations which do justice to all the branches without being too heavily loaded from the point of view of one. But when we come to the third aspect we find that this has hardly been developed at all, most men of science pay very little heed to it, or, more commonly, are not even aware of its existence, still less of its importance. Here again physics

is superior to biology, although the standpoint of the critical aspect is by no means fully understood by laboratory physicists. And yet to my mind this aspect is just as important as any of the others, and certainly just as important as the second, because the speculative aspect is subordinate to both investigation and criticism. It has to do with superstructure which is always liable to revision, either as a result of further investigations or as a result of deeper critical insight. Facts, on the other hand, provided they *are* facts, always stand, whatever becomes of the theories founded upon them, and the critical aspect also has to do with foundations—not by way of positive addition of facts, but by way of removal of obstacles, false assumptions, fallacious inferences, discovery of the influences of subjective factors, and so on. But it also has its positive or constructive contribution to make. Looking at old problems in a new light, or from an unfamiliar point of view, usually brings fresh insight, and the clear formulation of a problem is the first step, and indeed the most important one, towards its solution. In this book I have brought forward reasons for believing that the neglect of this critical aspect is partly responsible for the unsatisfactory development of present-day biology as contrasted with some other sciences, and these considerations suggest that it is in this direction that there is scope for improvement in the training of biological students.

But while, as I say, I have been trying to close the breach between the natural and the philosophical sciences, I have at the same time been trying to distinguish them more clearly. We have to learn to distinguish without separating. We require a division of labour, but not a division of the spoils. I have shown how little biology has clearly distinguished the metaphysical from the scientific attitude towards problems. Metaphysics interpenetrates biology in its speculative aspect, just as logic and epistemology interpenetrate it in its critical aspect.[1] Metaphysics and logic only differ from speculative and critical biology in the generality of their standpoint. The relation between a given branch of science and metaphysics is an asymmetrical one. By this I mean that metaphysics is dependent upon all the special sciences for its data, but the

[1] It seems probable that theoretical biology is also "interpenetrated" in its "teleological" aspects by another philosophical science, namely, theory of values. See the book by R. B. Perry quoted above on p. 430.

special sciences ought not to be influenced by metaphysics, still less should they dictate the form metaphysical interpretations should take. And when I say that the special sciences should not be influenced by metaphysics, I mean that they should not borrow notions from metaphysics blindly. We have now seen many instances of the blinding influence of metaphysical notions on natural science, resulting from the fact that those who borrowed them have not always understood their nature. And when metaphysics takes its data from a given science it ought to bear in mind the state of development of the science in question. Biological notions are beginning to play a great part in metaphysical speculation. But it is obvious that the present state of biology is one which not only might justly be described as primitive, but it has, as we have seen, been very largely developed under the influence of a one-sided metaphysic of nature. If this is the case the generalizations of biology are likely to be too unripe to be of much value to metaphysics. And since the critical aspect of biology has been so little explored it is to be expected that it is here that most attention is needed, and that when such a purification process has been more fully carried out it will have important consequences for the speculative aspect of biological science. It is with the critical aspect of biology that I have been exclusively concerned in this book. But logic with epistemology, as one of the philosophical sciences, is not at all concerned with the special difficulties of biological knowledge, and those logicians who have interested themselves in the methodology of natural science have usually confined their attention to physics. Consequently the only help we can obtain from logic and epistemology is of a very general kind, or of a kind which has chiefly been developed for the benefit of the physical sciences. All the recent developments in logic and epistemology need therefore to be understood with a view to their exploitation for the benefit of biological thinking. In this chapter I shall try to summarise the results of some of the foregoing discussions, and point out the directions in which there is reason to hope for good results from further study.

For the purposes of this summary we can group the characteristics of modern thought which are especially relevant to

our problems under three heads, representing contributions by: (1) Logic and epistemology; (2) Physical and psychological science; (3) The 'organic view' of nature, or the modern cosmological outlook.

(1) The most interesting development of modern thought in relation to theory of knowledge is the return to realism, but to forms of realism which are as shocking to common sense, or naive realism, as any of the older epistemological theories. We have seen how biological discussions are apt to be conducted either upon a basis of naive realism, or under the influence of those epistemological notions which flowed from the treatment of the secondary qualities as 'unreal'. The former state of affairs leads to our taking common-sense notions as being more concrete than they in fact are, and leads to the 'fallacy of misplaced concreteness'. We saw examples of this in the use of the cell-concept, and in the notion of 'structure' anatomically conceived. The second alternative results in the unfortunate habit of treating hypothetical postulated entities as 'more real' than that which they have been devised to explain. This has been the commonest method of treating change, and we have seen examples in the use of hypothetical particles (pp. 280, 331). Imperceptibly this has had the result of leading natural science away from its proper empirical attitude, and it is the return to this—the return to what can be perceived—which is one characteristic of modern physics.

The other tendency I have tried to rebut is that which can be described as 'phenomenalistic'. In so far as this represents a return to what can be perceived this is a good tendency but, in the forms we have examined, it does not seem to offer a tenable position as a basis for science. We have noted two forms of this movement: that represented by Mach and his followers, and the 'Bradleyan' form, both of which have their ramifications in biology, especially the latter, which is the point of view held by the methodological mechanists. I have given reasons for believing neither of these positions to be satisfactory, and I have tried to develop a realistic doctrine of knowledge along somewhat similar lines to those pointed out by Prof. Whitehead. This seems to have worked tolerably well, but I should be sorry to give the impression that I suppose it to be either so simple as it may have been made to appear, or beyond the necessity for

further development and critical testing and revision. Some obvious difficulties have been treated too lightly—particularly those relating to the apprehension of space-time. We are also greatly indebted to Professor Whitehead for his elucidation of the nature of abstraction in natural science, and I have drawn attention in many places to the fallacious consequences of not recognizing the nature of the abstractive process in biology. A common mistake consists in making a false antithesis between abstract and real, an instance of which we saw in the passage quoted on p. 232 where 'mere impression of the mind' and 'convenient abstraction' were contrasted with 'having a real existence'. It is also commonly believed that the abstractive process is an arbitrary one, whilst in contradiction to this is the equally common tendency to forget the existence of abstraction, to take abstracta as concrete, and to suppose them to be exhaustive. This brings us to another important modern tendency which can be described as the abandonment of 'absolutism' for 'relativism', and coupled with this is what we may call the 'recognition of the importance of the possible'.[1] From the former tendency we learn the lesson that alternative methods of abstraction are mutually complementary rather than mutually exclusive. Only contradictories are mutually exclusive. But it is evident from the nature of abstraction that what one method omits another method must include, if what is omitted is important. There is room for *both* 'mechanists' *and* 'antimechanists' in biology, as representing *different* modes of abstraction, so long as they attempt to understand and co-operate with one another, instead of wasting their time by efforts at mutual extermination. But so long as the devotees of this science are divided into conflicting sects which conduct their debates like rival missionaries—appealing to 'subjective-factors' by means of *ad hominem* arguments, rather than to reason, so long will biology continue to be a 'science of antitheses', and effort which might be more helpfully employed exploring *other* possibilities will have to be

[1] *Cf.* B. Russell: 'Logic, instead of being, as formerly, the bar to possibilities, has become the great liberator of the imagination, presenting innumerable alternatives which are closed to unreflective common sense, and leaving to experience the task of deciding, where decision is possible, between the many worlds which logic offers to our choice.' *Problems of Philosophy*, p. 131. See also the Herbert Spencer lecture reprinted in *Mysticism and Logic*, especially pp. 111 *et seq.*

devoted to removing needless and artificially created difficulties. This leads us to the second tendency which invites us to turn a more tolerant eye towards the exploration of the realm of possibility, with a view to finding alternative interpretations. One of the most deadly consequences of the cocksure and dogmatic attitude of natural science during the last century was the way it led people to believe that we had learnt all that there was to know *in principle*, and that the future had no surprises in store for us. Modern physics has found reason to depart from this attitude, but it still seems to be operative in biology (p. 255). This too is another instance of the departure from the empirical attitude.

Among the most remarkable developments of modern thought which fall under the present head are those in formal logic which have emerged in the course of investigations on the foundations of pure mathematics. It has been shown that the latter is itself a special development of formal logic, and other symbolic techniques have been developed of a wider and different scope. It is possible that this symbolic logic may some day be of service in dealing with biological problems. But in regard to the use of any deductive logic it is important to understand that all it does is to provide a technique for working out the consequences of a theory or system of premises from which we start, and in natural science these will always involve reference to what is observed. And symbols are useless if you have no clear concepts of what they symbolize. 'A symbol which has not been properly defined' writes Prof. Whitehead, 'is not a symbol at all. It is merely a blot of ink on paper which has an easily recognizable shape'.[1] But we have seen that a great many of the most important terms in biology are commonly used without adequate definition, and we have seen some of the consequences of this. A great number of the words used in biological books are nothing more than ink marks without a clear meaning, and it is because no one considers it necessary, either to assign a clear meaning to such terms, or to abandon them altogether, that much biological controversy is apt to be so fruitless. But so long as this is the state of affairs there is no hope for clear thinking or for the application of any sort of symbolic technique. Neither is it of any avail to accumulate measurements unless you have clear concepts relating to what you are

[1] *Introduction to Mathematics*, p. 91. See also pp. 92 and 127.

measuring. If we are to be guided by the history of physics and chemistry we ought to seek for the most pervasive and most constant features of organisms and to try to find some measurable character related to them. One most pervasive feature is the cell-type of organization. Another is the constancy of specific characterization which is studied in genetics. But the feature which is so characteristic of biological organisms is the fact that their mode of persistence is 'by means of' change, not 'by absence of' change. It seems to be this fact which is responsible for the peculiarities of the relation of the organism to its environment, and of the parts to the whole, and it is difficult to see how any theoretical biology which refuses to take this into account can be satisfactory. It remains to be seen how far such requirements admit of mathematical or symbolic treatment, although it would seem that it is in these directions that we should look for the missing concepts which are to play the same rôle in biology as acceleration does in physics and combining proportions in chemistry. But before we reach this stage there is an enormous amount to be done by way of purifying and making clear the biological concepts which we already possess, and which have already done good service. Charles Darwin has been called the Newton of biology, but it will be time enough to talk about the Newton of biology after our science has found its Galileo. To suppose that Darwin was the Newton of biology is to suppose that biology has already reached a degree of theoretical development comparable with that of physics in the eighteenth century, and that surely is preposterous. We only make our great men ridiculous by putting them in fancy dress in this way, and we flatter ourselves with a baseless complacency if we imagine that our science has attained to a greater degree of development than is in fact the case.

Finally, under this head come also the criticisms to which the notions of substance and causation have been subjected during recent years. It is sufficiently clear that the old Aristotelian notion of a thing with attributes, and the notions about causation which we carry over into science from common sense, are full of difficulties in their use in theoretical biology. From what has been said on these points it is evident that here is an important 'growing point' of biological methodology which deserves more careful attention, particularly in relation to development and 'adaptation'.

(2) Under our second head, namely contributions of the empirical sciences, undoubtedly the most important from our point of view is the assimilation of space and time, and the necessity of 'taking time seriously' which is characteristic of modern physical science. It is this, coupled with epistemological criticism, which is so largely responsible for the departure from the old-fashioned scientific materialism. I have tried to show how important this is for biological thought, especially in relation to the time-honoured antithesis between structure and function. This antithesis seems clearly to rest partly on taking structure too naïve-realistically, and partly on the separation of space and time. The notion of the space-time event seems to offer a means of completely overcoming this antithesis, and I have tried to suggest some of the consequences of its removal, particularly in relation to developmental and 'teleological' questions, but it is evident that these consequences should be far reaching and their complete working out requires and will repay a great deal of careful consideration. Clearly one of the most interesting and important problems for the biology of the future is that of the relation of 'characters' to 'parts', of genetic 'factors' to developmental process, of the persistent racial immanent endowment to the process in which it is displayed in the course of temporal passage.

Two features of modern psychological science are worth mentioning under the present head. The first is the *Gestalt-theorie*, which is of interest in so far as it represents a reaction against the old atomistic tendency in psychology. The ideas employed and the methodological points involved may be of interest from the biological standpoint also.[1] The second feature is the rapid development of the schools of psychological medicine which owe their inspiration to Sigmund Freud, however much they may differ from him in content. This development is also of interest from the methodological standpoint because it shows the possibility of developing a systematic science without the use of the traditional concepts which have reigned so long in natural science as to be considered inseparable from the very name of science.

(3) The third and last head—the 'organic view' of nature

[1] For a short discussion of this theory from the biological standpoint and full references see Bertalanffy's *Kritische Theorie der Formbildung*, Berlin, 1928, pp. 106 *et seq.*

—which is championed by Professors Whitehead, Lloyd Morgan and others, is of no direct concern to biology in so far as it deals with the wider metaphysical cosmological problems, but it is of importance from the biological point of view in its emphasis on the concept of organization, and on relations—especially internal and multiple relations. It is the recognition of the importance of multiple relations which makes the Aristotelian logic of subject and predicate so inadequate. In this book I have tried to analyse the notion of organization in relation to the actual problems of biology (a task which, so far as I know, has not been attempted by biologists), with the aids provided by modern thought, but here again I do not profess to have done more than open up the subject. There is good reason to believe, however, that insufficient attention to these points has had unfortunate results for biological development, and that here also there is room for further inquiry. In this connexion too, I have tried to discover the exact respects in which Dr. J. S. Haldane differs from the majority of physiologists. And because Dr. Haldane's view of the organism seems to me to be good (apart from whatever wider metaphysical opinions he may hold) I have attempted to discover and explain what that view is, and to remove the objections which physiologists have brought against it and the misunderstandings upon which those objections appear to rest.[1] I have emphasized the importance of the notions of part and whole in biology, and attempted to clear away certain misunderstandings which have gathered round the cell concept.

[1] It is a misfortune that Dr. Haldane has never expounded his views more systematically and precisely. Much that he says is extremely vague, and this applies also to his Gifford Lectures, which have appeared while these last pages were passing through the press. Consequently I am doubtful how far Dr. Haldane would agree to my interpretation. I fail to see how his arguments refute mechanism in every form. It is always possible to defend microscopic mechanism in principle, if any one wishes to do so, by making your mechanism complicated enough, and by 'postulating' enough sub-mechanisms to meet all contingencies. It cannot then be refuted, but neither can it be verified. All I have undertaken to do is to show the undesirability of restricting biological thought in this way, and the desirability of studying organisms at all levels of organization as such, in terms of a hierarchy of relata in their organizing relations in virtue of which the organism tends to persist. This covers most of Dr. Haldane's contentions. I fail to see how Dr. Haldane escapes from causal relations and thus from 'mechanistic explanations' in this most general sense (see pp. 258 and 441 above). But he does not appear to distinguish between the many different kinds of 'mechanistic explanation'.

And I have tried to develop a new interpretation of the cell concept in accordance with the refusal to separate time and space. We must try to think four-dimensionally and regard the cell concept as expressing a pervasive type of spatio-temporal organization which may characterize an organic part or a whole, and of which the cytological and metabolic aspects are two different aspects reached by different modes of abstraction. This, if we omit the bacteria, is a level of organization below which the 'rhythm of life' never falls so long as genetic continuity is preserved, but which characterizes most organisms either throughout the history, or during the earlier periods of every rhythmical 'repeat'. Moreover, if the doctrine of evolution as an epigenetic process is true this has been the type of organization upon the spatial repeatability and temporal differentiation of which every evolutionary achievement subsequent to it has depended. It thus deserves to be numbered among the most permanent and 'versatile' types of organization in the world.

These, then, are some of the contributions which modern thought has to make to biology. But they cannot be utilized unless biologists are prepared to undertake something in the nature of an intellectual stock-taking, and this, for the reasons given in Chapter IV, Section 3, is difficult and painful—much more painful than having a tooth extracted because it cannot be done by anyone else and it cannot be done under an anæsthetic. What biology requires is a better ventilation of its thought and a more critical scrutiny of its concepts; more openmindedness and more careful consideration of the relation between investigation and theoretical interpretation. But above all we need a wider recognition of the value of thought itself. Biologists have taken the celebrated saying of John Hunter: 'Don't think; try' too literally. This is not the maxim upon which physics was founded. You must think first and *then* try. And you must think about the right things. In biology we require to think primarily about biological facts, not about hypothetical billiard balls. But even thinking about biological facts is not enough. We must scrutinize our ways of thinking too, in order to try to overcome the limitations of those two grooves into which thought has been confined since Descartes, from which it has resulted that a specifically biological way of thinking has hardly been so much as considered. We also require to develop a rather

greater sensibility to logical contradiction. We have seen many instances of the way in which, unless a contradiction is as blatant as a bad smell, it passes unnoticed. But in proportion as a science lacks unity and 'deductive power' so also will opportunities be lacking of discovering contradictions between the results of its various branches, and thus of employing one important means of discovering error.

I began in the Introduction by pointing out the lack of unity between the various branches of biological science. We have now seen how numerous and varied are the factors upon which this lack of unity rests. It has been shown how largely epistemological and, to some extent, psychological, factors are involved in this lack of unity, apart from the difficulties inherent in the subject-matter itself. Some few of these difficulties it has been possible to remove, but a very great deal still remains to be done in this direction. The progress of a science requires the proper parallel development of all its three aspects—the investigatory, the speculative and the critical. In biology the most advanced at present is the investigatory aspect. Its speculative aspect has been overshadowed by notions uncritically borrowed from extra-biological sources. But the accumulation of data, and the use of hypotheses borrowed from elsewhere is not enough. We need to give some attention to the neglected critical aspect in order to make us less self-satisfied, less timid, and more independent. At present the application of logical and epistemological principles to the critique of biological knowledge is not at all understood in the English-speaking countries. Since our biologists are only trained to be good investigators we can hardly expect them to be good thinkers. It does not seem necessary that everyone who wishes to pursue biology should receive a stereotyped training. Specialization seems to be as unavoidable here as elsewhere. The types of mind needed for investigation and for criticism appear to be very different, and provision should be made for the encouragement and training of both. There are probably good psychological reasons why disinterestedness is neither possible nor desirable in the investigator. 'If you want an absolute duffer in an investigation' wrote William James, 'you must, after all, take the man who has no interest whatever in its results: he is the warranted incapable, the positive fool'. But the capacity for disinterested reflexion which

makes a fool of the investigator is the saving grace of the critic. Accordingly, if we are to develop this aspect of biological inquiry, provision ought to be made for the help and encouragement of those students whose gifts and inclinations lie in that direction. When, therefore, Prof. Wilson says that 'Knowledge will be advanced most surely by assuming that the problems of the cell can be solved by converging upon them all our forces of observation and experiment', he omits one important ingredient, namely thought, and under thought I include not only critical penetration but also creative imagination.

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C. K. OGDEN, M.A.

Magdalene College, Cambridge

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